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OBSERVATIONS

ON THE

NATURE AND CURE OF

CALCULUS, SEA SCURVY, CONSUMPTION, CATARRH, AND FEVER:

TOGETHER WITH

CONJECTURES UPON SEVERAL OTHER SUBJECTS OF

PHYSIOLOGY AND PATHOLOGY.

By THOMAS BEDDOES, M.D.

LONDON:

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M.DCC.XCIII.

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TO THE

DISCOVERER

OF THE VIRTUES OF

VEGETABLE ALKALI, SUPERSATURATED WITH CARBONIC ACID.

SIR,

IT has been frequently with great confidence affirmed, that our acute pains are of ſhort duration. A very ſlight acquaintance, however, with the tremendous catalogue of human maladies, will ſatisfy us that this is the vain aphoriſm of a ſophiſt, more anxious to place words in oppoſition, than to obſerve the courſe of nature. Our excruciating diſeaſes are, if I do not compute very much amiſs, remarkable for length of paroxyſms, and for frequency of recurrence; while in thoſe of a different character, languor and depreſſion are ſcarce leſs intolerable than the moſt intenſe pain.

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I hope, and I believe, that this mighty maſs of evil will be gradually diminiſhed, and finally diſappear from the face of the earth. We are juſt beginning to catch a glimpſe of the laws of animal nature; and now, when the human mind ſeems, in ſo many countries, about to be rouſed from that torpor, by which it has been ſo long benumbed, we may reaſonably indulge the expectation of a rapid progreſs in this, the moſt beneficial of all the ſciences. An infinitely ſmall portion of genius has hitherto been exerted in attempts to diminiſh the ſum of our painful ſenſations; and the force of ſociety has been excluſively at the diſpoſal of Deſpots and Juntos, the great artificers of human evil. Should an entire change in theſe two reſpects, any where take place, every member of ſociety might ſoon expect to experience, in his own perſon, the conſequence of ſo happy an innovation; and ſhould the example be generally followed, there is no improvement in the condition of the World, for which we might not hope from the bloodleſs rivalſhip of nations.

From

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From Chemiſtry, which is daily unfolding the profoundeſt ſecrets of nature, and, among the reſt, the delicate play of living machinery, your example alone would juſtify us in entertaining the moſt ſanguine expectations: ſince the earlieſt diſcoveries in that department of chemiſtry, which has been ſo ſucceſsfully cultivated by BLACK, CAVENDISH, PRIESTLY, SCHEELE, and LAVOISIER, ſuggeſted to you a ſafe and efficacious remedy for one of the moſt frequent, painful, and hopeleſs of diſeaſes.

Much as you have contributed, by the frank and diſintereſted communication of your diſcovery, to obliterate one of the darkeſt ſhades from the proſpect of life, your name is, I ſuſpect, ſcarce known beyond the narrow circle of the practitioners of medicine, except, perhaps, to a few among thoſe who are indebted to you for eaſe and health. Such is the inattention of mankind to their beſt benefactors! and ſo entirely have fatal illuſions perverted our moral ſentiments! I cannot hope to add much to your

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reputation; but by attempting to diffuſe more widely the benefit for which mankind are originally indebted to you, I may perhaps afford you gratification.

That the former part of the following pamphlet will do ſome good, I am confident; though I do not believe that alkaline medicines will relieve calculous diſorders under every form. Thoſe diſorders, beſide the different ſeats they occupy in different perſons, appear alſo, from the analyſes of various calculi, to be liable to conſiderable variation in their nature.

The ſpeculations that follow, will, perhaps, appear to you too remote from application, and my hopes of the future improvement of medicine too high-flying.

It is, I am ſenſible, but a poor expedient, to lay one's ſelf out for the praiſe of ingenuity by propoſing projects which are in no danger of being diſgraced by trial; nor have I ever much regarded medical obſer-

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vations, that are of no other uſe than to be read. But this, I can aſſure you, ſhall not be the caſe here; and the more I reflect, the more confident I become, that an eaſy and convenient method of offering phthifical patients a chance of recovery, which has never yet, upon any probable grounds, been offered them, will ſhortly be contrived. For *typhus*, if the light that is now dawning upon phyſiology and pathology does not preſent objects to me under very illuſive forms, we ſhall not fail to ſtrike out an almoſt infallible method of cure; and this method, I think it probable, will extend to the ſcarlet fever alſo; which is perhaps the moſt formidable among the acute diſeaſes of this climate. In the treatment of fevers we have, it is true, learned to avoid ſome fatal miſtakes of our anceſtors; but we can boaſt of little elſe. In thoſe caſes in which alone there is, perhaps, occaſion for the interference of art, art ſeems almoſt impotent: from attention to the ſingle circumſtance of debility, I imagine, that patients are often drenched with wine and opiates, till they are ſtimulated to death. If I have

imputed the debility to its real cauſe, our chief aim ſhould be to reſtore the principle of excitability; and ſtimulants ſhould in the mean time be adminiſtered with a more ſparing hand. Perhaps, when the proper method of reſtoring this principle ſhall have been deviſed, *extraordinary* ſtimulants will become unneceſſary. The Materia Medica was once ſuppoſed to contain diſtinct ſpecifics for the diſeaſes of each ſeparate organ; it is now regarded as little elſe than a collection of ſtimuli; ſo that medicine is become the art of adminiſtering drams. Hence it can often only amuſe or palliate, and muſt ſometimes injure, by forcing into motion, conſtitutions already too much worn. How would our reſources be multiplied, if we could give excitability or life, as well as ſtimulants! "But "is ſo ſalutary a revolution in medicine "poſſible?" I do not know; but is it not worth while to enquire?

I am, SIR,

Reſpectfully your's,

Oxford,
30th July 1792.

THOMAS BEDDOES.

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AN

AN

ACCOUNT

OF A

SIMPLER METHOD OF TREATING CERTAIN

CALCULOUS COMPLAINTS.

BOOKS and tradition never fail to offer a multitude of medicines for diſeaſes that are frequent and incurable; many of theſe medicines are the ſuggeſtion of the moſt fantaſtic analogies, and the greater part are incapable of even palliating for a moment the ſufferings of the patient; yet a liſt, at firſt ſight ſo unpromiſing, is not abſolutely without its uſe. The phyſician ſtalks abroad with an air of greater dignity when he feels a full quiver at his ſhoulders, however blunt may be the arrows it contains; and it ſupplies a ſtaff,

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however feeble, on which the wearied ſpirits of the patient may reſt, and defers a little that ſeaſon of ſettled gloom when Futurity has nothing to promiſe to Hope.

There exiſts, probably, no human malady, not even the jaundice, conſumption, ague, cancer, or dropſy excepted, for which ſo many whimſical and nugatory means of relief have been propoſed, as for the ſtone and gravel. Beſides an infinity of inefficacious ſimples, the whole ſeries of remedies, from the warm goat's blood of Alexander Trallianus, the pounded glaſs of Baricellus a Sancto Marco, (*a*) the eſſence of pigeon's dung of Johannes Poppius, the *quinta eſſentia urinæ humanæ* of Fabri, the *ſpiritus microcoſmi e ſtercore humano* of another chemical or alchemical

(*a*) *Incredibile nec forté unquam tentatum remedium*, ſays Dr. Siebold. Yet the powder occaſionally found within the cavity of flints is ſtill uſed as a remedy for calculous complaints in our chalk counties; I met with an inſtance of this kind within theſe few days. It was probably conceived, in both caſes, that one kind of grit would draw or drive the other out of the body.

doctor,

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doctor, down to the bearberry of De Haen, at once afford a proof of the inefficacy of each particular medicine, and of the prevalence of a diſorder which could enforce ſo much attention, and ſuggeſt ſo many extravagant projects.

From all the teſtimonies that have fallen under my obſervation, I can collect, that during the former part of the preſent century ſome approaches had been making towards a remedy, which, whatever may be its mode of operation, or preciſe degree of efficacy, is undoubtedly capable both of relieving that pain, which renders the diſorder ſo formidable, and of ſuſpending the progreſs of the diſeaſe itſelf. As early as the year 1721, Robinſon propoſed ſalt of tartar, among other things, as a ſolvent for the ſtone (*b*). In diſorders of the urinary organs, whether ariſing from concretions or not, Hoffman praiſes the efficacy of the hot alkaline ſprings of Germany, as

(*b*) Treatiſe of the Stone and Gravel, 1721.

well as of the ſalt obtained from the waters of Carlſbad. At a later period, alkaline ſubſtances enriched the Engliſh empirics, and obtained the commendation of Hartley, Whytt, Kirkpatrick, De Haen, and other phyſicians of great celebrity. It appeared, indeed, that the concretions were not really diſſolved, even in the more favourable caſes; yet the pain was permanently relieved. Every phyſician is acquainted with the very curious facts on this ſubject related by Whytt, and more eſpecially by De Haen (*c*). Yet, notwithſtanding ſome partial ſucceſs, theſe cauſtic materials were afterwards generally

(*c*) See the firſt volumes of De Haen's Ratio Medendi. The caſe of that patient who had ſwallowed eight hundred quarts of lime-water in half a year, and who continued free from pain for three years, though he had ſtill a ſtone in his bladder, is very remarkable. The ninth caſe, related in the "*Account of the Efficacy*," &c. affords another ſuch inſtance of palliation. The patient appears to have been kept more than tolerably eaſy by the ſuperſaturated ſolution for two years and an half; yet, from the concluding expreſſion of his letter, "*I am ſeldom troubled with any pain*," I infer that the diſeaſe ſtill exiſted in the bladder.

laid

laid aſide. They were, however, laid aſide with regret, ſince this inference ſeemed to be warranted by the whole ſum of facts, that much benefit might be derived from them, provided their collateral bad effects could be obviated. This concluſion appears evidently to have reſted upon the mind of Profeſſor Bergman, who, as far as this diſorder is concerned, may be quoted as an authority in medicine: in a paper publiſhed in 1776 he has theſe words—"*hoc unum addo, calculi analyſin chemicam medicæ arti haud parum utilitatis polliceri, calcis vivæ aquam et lixivium cauſticum alkalinum calculo mederi experientiâ conſtitit. Idem vero ſi ignotum hactenus fuiſſet ex ipſâ calculi mixtione nunc inventâ et detectâ intelligeretur.*"

It was reſerved for a reſpectable member of the medical profeſſion, ſtill living, to engage the modern chemiſtry in the ſervice of medicine, and realize a project which now ſeemed to be relinquiſhed in deſpair. This gentleman's reflections were quickened by his own feelings, and in

1778, after having been for eighteen years ſubject to ſevere nephritic paroxyſms, he began to take a ſolution of fixed vegetable alkali, ſuperſaturated with carbonic acid, or fixed air. This medicine very ſoon relieved his calculous ſymptoms, and, as it appears from the account of his caſe, has kept him free from pain for ten years, one ſlight attack excepted, which is aſcribed to the diſcontinuance of the medicine for ſeveral weeks. Perhaps it might be ſerviceable to mankind, if medical practitioners, attentive to the progreſs of ſcience, and capable of combining ideas, were from time to time to be ſeized by thoſe diſeaſes for which remedies are ſtill wanting. The narrative of this caſe, with that of upwards of twenty others, is contained in a pamphlet, now well known under the title of "*An account of the Efficacy of Aqua Mephitica Alkalina, &c. in Calculous Diſorders.*" Experience has ſince amply confirmed the virtues of a medicine, which, I apprehend, may be freely taken without danger, and even without inconvenience, except in a few

rare

rare inſtances, and which ſeems to have deſerved the ſingular praiſe of equalling the expectations raiſed by the perſon who firſt propoſed it. The method of preparing this medicine is as follows: Diſſolve two ounces and a half (troy weight) of dry ſalt of tartar in five quarts (wine meaſure) of ſoft water; after ſtirring the water, and then ſuffering it to ſtand long enough for the ſubſtances generally precipitated from water by fixed alkali, and the reſiduum of the ſalt of tartar itſelf to ſubſide, pour off the clear ſolution, and place it in the middle veſſel of Parker's apparatus for impregnating liquids with fixed air, and expoſe it for forty-eight hours to a ſtream of that elaſtic fluid. Of this liquor, from twelve to twenty-four ounces have been taken every day by different perſons afflicted with various calculous complaints, and always, except in one inſtance, with the deſired effect, after it has been continued ſome time.

One might, perhaps, be diſpoſed to wiſh that this remedy could be prepared with

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leſs trouble and attention; but the great deſideratum is a cheap, ſafe, and efficacious formula, adapted to the poor, who are by no means exempted from calculous diſorders; for when the high price and brittleneſs of the apparatus is conſidered, and when we likewiſe take into the account the neceſſity of conſtantly continuing the medicine, in order to prevent the return of the diſeaſe, it will appear probable, that the poor are not often likely to reap the full benefit of the diſcovery.

In the year 1786 or 1787, a perſon belonging to the medical profeſſion, and much afflicted with the gravel, complained to me that he was unable to perſevere in the uſe of aqua mephitica alkalina, on account of the great dizzineſs it always occaſioned (*d*). I was

(*d*) Dizzineſs with a degree of intoxication, is a very common effect of liquors containing carbonic acid. Pyrmont water, which contains of carbonic acid air conſiderably more than a quantity equal to the bulk of the water itſelf, occaſions a glow, exhilaration, and confuſion

I was led by this intimation to reflect upon the ſubject, and after ſome time fell upon what indeed was abundantly obvious; a formula of which I think myſelf fully warranted in aſſerting, that it is extremely beneficial in calculous complaints, and that it may, without injury, be taken in very large quantities, and continued for a great length of time. Its ſimplicity and its cheapneſs are its great recommendations. I cannot determine, for want of comparative ob-

fuſion of ideas ſimilar to that which follows the uſe or abuſe of ſpirituous liquors. This effect is ſo common as to have given riſe to a particular term, *brunnen rauſch*. A ſpring of the ſame quality, which the Tartars, on this account, call *the well of drunkenneſs*, is mentioned by one of the late travellers into Siberia. Some bottled liquors, I ſuppoſe, in part, owe their intoxicating power, and more eſpecially the ſuddenneſs of their effect, to carbonic acid. This acid is produced in the bottles by a ſlow continuance of the vinous fermentation, and therefore theſe liquors will contain more alcohol when they are ripe, than at the time of bottling; but I do not imagine this difference will account ſatisfactorily for the prodigious difference of their effect upon the head. Sparkling Champagne probably owes its exhilarating power to the ſame cauſe.

ſervations,

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ſervations, whether it is inferior to the more operoſe preparation in efficacy; and how much inferior; there can be no reaſon to ſuppoſe that it is ſuperior: and were both equally eaſy to procure by all patients, I ſhould not think it worthy of public notice.

The formula I have employed for two or three years paſt is as follows: take natron or ſal ſodæ in cryſtals, pound it coarſely, and expoſe it to a warm dry air, till it entirely crumbles into a white powder; make this powder into pills with ſoap or any other cement; aromatics, extract of bark, &c. may be added; but I have never found any addition neceſſary; a quantity of ſoap, rather more than equal to the weight of the calcined alkali, is neceſſary to make it into pills (*e*).

In

(*e*) I now ſee that Dr. Donald Monro (*Materia Medica*, article Alkali, 1788), had an idea of giving foſſil alkali in pills, in calculous caſes, "It may," ſays he, and this is all he ſays, "be made into pills or boluſſes, "mixed

In order to expedite the expulſion of the water of cryſtallization, the alkali may be ſpread out before a fire, or the veſſel containing it may be placed in boiling water, and the pounded cryſtals ſtirred till they have loſt rather more than half their weight: the recent cryſtals contain indeed ſixty-four parts of water in an hundred; but unleſs kept cloſe, they loſe part of this water, and it is prudent not to carry the calcination too far, if artificial heat be applied; nor ſhould the heat exceed that of boiling water, leſt any of the volatile acid ſhould be expelled; of this powder from one to two ſcruples taken every day has generally af-

" mixed with ſome powder of liquorice root, by means " of gum arabic mucillage, or conſerve." From ſeveral articles of that work I had, perhaps haſtily, concluded that it fell ſhort both of modern chemical, and medical knowledge, and this paſſage had eſcaped me. I ſuppoſe the author, as he gives no intimation of the contrary, means to propoſe the cryſtallized alkali for pills. I have often found the pills apt to fall to pieces, when the water of cryſtallization is not ſufficiently expelled; I ſhould think this would happen ſtill more, when none is expelled.

forded

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forded relief in leſs than three weeks; and in no caſe but one, out of more than twenty that have fallen under my own obſervation, have they failed to perform every thing which could be deſired from medicine, except eradicating the tendency to form calculous concretions, to which no known remedy has the ſmalleſt pretenſions.

I might perhaps ſafely truſt to the abovementioned pamphlet, as bearing abundant teſtimony to the efficacy of alkaline ſalts; and confidently appeal to future experience in confirmation both of the power of ſal ſodæ in the form preſcribed, and of its harmleſſneſs; at all events it will be unneceſſary to particularize ſlight caſes, which have always at once yielded to the remedy. The following, in which the ſymptoms were either of very long ſtanding, or extreme ſeverity, will, I hope, be ſufficient to procure a trial to the medicine.

I, Mr.

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I.

MR. WILLIAM RUSHTON, of the Wyke, near Shifnal, Shropſhire, had been haraſſed for a conſiderable number of years by excruciating pains in his loins, attended with occaſional ſickneſs, and an almoſt total inability to ſtoop; the pains were accuſtomed to ſpread in all directions, and ſeverely to affect his head. He had at different times diſcharged much gravel; his urine formed depoſitions, was often extremely offenſive, and full of mucus; he was at times afraid to diſcharge it, ſo much were the paſſages irritated, and ſo intenſe the pain ſucceeding the evacuation. In July, 1787, he began to take a drachm of chryſtallized foſſil alkali, diſſolved in a quart of water, every day. In a few days he felt relieved, and in leſs than a month ſeemed, as he expreſſed it, *to be quite another man.* I have ſeen him repeatedly ſince; his ſufferings from his complaints have been very inconſiderable; but having ſometimes neglected the

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the medicine for months together, he has felt ſome ſtiffneſs rather than pain acroſs his loins, which has immediately been removed by a repetition of the alkali.

The ſolution ſometimes produced a ſlight nauſea, againſt which his dread of the pains determined him to bear up. The pills have never been attended with the ſlighteſt unpleaſant ſenſation. During his long experience he obſerved, that ſtale (acid) beer never failed to bring on a ſevere paroxyſm. Several other perſons have repeated the ſame obſervation. On the preſumption ariſing from this information, I have always enjoined abſtinence from malt liquor in that ſtate; no other particular reſtriction of diet has appeared neceſſary, yet the inhabitants of cyder countries, as I have been informed upon inquiry, are remarkably free from this diſeaſe. Do the native and acetous acids differ in their effects? I ſhould imagine not. The ſpeedy effect of ſour beer ſeems to ſhew that it does not act by producing new concretions, but by ſome irritating power.

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power. Would cyder affect a perſon ſubject to calculous paroxyſms in the ſame manner? Diuretics, as diluted ſpirits, generally do miſchief: the ſame remark, I think, occurs in the pamphlet quoted above. May, 1792, Mr. R. gave me the uſual favourable account of himſelf.

II.

BRAMAH, a workman in the foundery of Mr. Dearman, of Birmingham, was become quite emaciated, and unequal to his labour, from a gravelly complaint, under which he had laboured many years. He had long been accuſtomed to diſcharge concreted matter, and ſmall ſtones; his urine depoſited an incruſtation, and ſoon became fœtid; the pains at his loins were intenſe, &c. &c. He began to take the ſolution as before in October 1787, found relief in a few weeks, and ſoon conſidered himſelf as radically cured. Notwithſtanding my repeated admonitions, he has at times neglected to continue the medicine, and has had returns

of his ſymptoms. I never have ſeen an inſtance which more evidently ſhewed how ſoon we forget the moſt acute pain.

Having ſo frequently experienced the efficacy of the medicine, he now chooſes to ſuffer the diſeaſe to return in a ſlight degree, and then for two or three days to take a *handful* of pills; the conſequence of which is a diſcharge of gravel, after which he feels no farther inconvenience for months. I have not been able to diſſuade him from this violent method of treating himſelf; he has not, however, ſuffered from it.

My friend, Mr. I. Dearman, in a letter dated May 2, 1789, gave me the following account of one of his relapſes, for he has ſo often relapſed and recovered, that his caſe alone is equal to half a dozen, in proof of the power of the remedy; " He began " with half an ounce of ſal ſodæ, with as " much common ſoap as with the addition " of a little gum arabic made ſixty pills. " On ſecond day (Monday) he took four,

" increaſing

'increaſing the number every day, till 'yeſterday he took twelve, without the 'leaſt affection of the ſtomach or increaſe 'of urine; nor can he give any other 'account of their effect, than that the 'pain is removing very faſt, and he does 'not doubt but a few more pills will cure 'him. I have not been without an ap-'prehenſion of danger from his taking 'them ſo largely—he treats my remon-'ſtrances very cavalierly, and ſays he 'ſhould not mind taking a box full of 'ſuch pills. He does not recollect any 'increaſe of urine when he was taking 'the medicine before.' Mr. Biddle, chemiſt, of Birmingham, in a letter dated June 1792, informs me, that 'Bramah 'and Wilks, at the foundery, have con-'ſtantly taken the pills, when they felt 'the complaint coming, and have as uni-'formly been relieved.'

III.

WILKS, a fellow-workman with Bramah, and not quite ſo terribly afflicted

with nephritic ſymptoms, found perfect relief from the pills. After every other diſagreeable feeling was removed, he continued for ſome time to have a ſtarting of one of his thighs. Suſpecting this to ariſe from ſome cauſe of irritation lodged in the urinary paſſages, or at leaſt to have ſome connection with his calculous diſorder, I adviſed him to perſevere ſteadily in the uſe of the pills; and he has ſince informed me, that he is no longer diſturbed by this involuntary motion of his muſcles. He alſo informed me, that he had cured two or three other perſons with his ſpare pills, of which he ſeemed very deſirous to know the compoſition, having ſome idea, as I ſuſpected, upon the ſtrength of his experience of their virtues, of trying his fortune in the practice of medicine.

IV.

DECEMBER, 1788. John Bucknall, labourer, of Kemberton, near Shifnall, Shropſhire,

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ſhire, has had the gravel for thirty years, and very violently for the laſt ſix or ſeven years. In harveſt the pains at his loins have always been moſt ſevere, eſpecially when he was reaping or mowing, a kind of labour which his diſorder has often rendered him incapable of following. The harveſt beer is ſeldom without ſome degree of acidity, to which he imputes, in a great meaſure, the ſeverity of his ſufferings at that particular ſeaſon. The attitude of a mower or a reaper may, however, alſo account for this aggravation. He was directed to take a drachm of cryſtals of ſal ſodæ diſſolved in three half pints of water. I believe, however, he much exceeded this doſe, as the common people are ſo apt to do. In three days he perceived an increaſe of his urine, and a great deal of red gravel came away. He had now the ſenſation of a large lump in his right groin, which in about nine days totally diſappeared.

January 11, 1789. He conſidered himſelf as totally cured. He was ordered to

take every day, for a month, a ſcruple of the ſalt diſſolved in water, at two doſes, and afterwards to repeat the medicine in the ſame doſe, one week at leaſt in every month; and if he found the quantity too ſmall to prevent the return of his diſorder, to regulate the treatment of himſelf according to his feelings, which a little experience would ſoon enable him to do.

I did not ſee him again till May, 1792, when he told me that he had long totally diſcontinued the ſolution, and that he had been perfectly free from any gravelly ſymptom till within a few weeks, when he perceived a ſtiffneſs in his loins, and ſome other indications of a return of his diſorder. I recommended to him the pills, but have heard nothing further concerning him. I met with one patient who would not exchange the ſolution for the pills. He is the only perſon I have ſeen, who thought the taſte of alkali not diſagreeable.

V. The

V.

THE caſe of Mr. Roe, timber-merchant, at Newport, in Shropſhire, was remarkable, on account of the violence of the ſymptoms, and the extreme irritability of the patient's habit. He complained of violent pains acroſs his loins, accompanied with ſickneſs and vomiting, and intolerable ſufferings immediately after the diſcharge of his urine, and a conſtant irritation of the bladder and urethra. He had from time to time diſcharged much gravel, and many calculi as large as horſe beans. He was not able to ride without making bloody water. More than one of the former patients had mentioned this circumſtance; but I believe the coffee-coloured is often miſtaken for bloody urine.

Early in 1789 he took half a drachm of cryſtallized foſſil alkali, with three or

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four grains of cream of tartar (*d*) every day for three weeks. His pains were ſen-

(*d*) I then thought that the addition of a little cream of tartar to the alkaline ſolution, at the time of taking it, would gradually extricate ſome fixed air in the ſtomach, and ſecure the ſaturation. I even ſuppoſed that I might, by a few experiments, diſcover ſuch a proportion of theſe ingredients as would effect, without any apparatus, a ſuperſaturation of the alkali that ſhould remain uncombined with the acid of tartar. But the pills ſuperſeded the neceſſity of further reſearches. On the addition of five or ſix grains of cream of tartar to a ſcruple, or a ſcruple and half, of ſal ſodæ, diſſolved in twelve or fourteen ounces of water, cloſing the mouth of the phial, and inverting it in water, the alkaline ſolution acquires a much leſs diſagreeable taſte than a mere ſolution of equal ſtrength has, and at the moment of uncloſing the phial, an effort of protruſion is felt, and a few air bubbles aſcend. The neutral ſalt thus formed would itſelf be probably beneficial; I cannot believe, at leaſt, that the carbonic acid contributes, by any direct operation, to the cure or palliation of the diſeaſe, ſince cauſtic alkaline ſubſtances clearly ſeem, if they could be ſafely taken, to have equal efficacy with the carbonated; and whatever be the effect of this acid, ſome of theſe other acids, that are eaſily diſlodged from alkalis, would produce it equally. A reſpectable obſerver (Menghini, Comment. Bonon. v. 261, &c.) recommends Rochelle ſalt in calculous caſes.

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ſibly diminiſhed, and the pills were now ſubſtituted in the place of the ſolution. In a letter dated March 13, 1789, he deſcribes himſelf as taking the pills: 'And 'tolerably well all the time, with now and 'then ſome little pain, after which a ſmall 'quantity of ſmall gravel comes away.' In a letter, dated 12th April, 1789, Mr. Jones, who attended him, ſays, 'There is no doubt 'as to Mr. Roe's caſe; I am very happy 'to inform you that he continues much 'better.' He took the pills regularly, felt from time to time ſome ſlight pains or intimations of pain, after which ſome gravel was diſcharged. In April, 1792, he called upon me to inform me that he had enjoyed eaſe and a good ſtate of health ſince I had heard from him laſt.

VI.

THE following letter contains an account of a rare irritability of the urinary organs. The medical reader will certainly adopt the

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writer's opinion, that it is not a calculous caſe. I ought perhaps to have ſuppreſſed the concluſion of the letter; but as the fervour of expreſſion, doubtleſs, correſponds to the pain the writer was once accuſtomed to ſuffer, and the relief he afterwards experienced, it is properly to be conſidered as deſcriptive of the caſe, eſpecially as I have not the ſatisfaction of knowing the perſon who gives ſo much credit to the medicine:

'SIR,

'At the requeſt of my brother-in-law, 'Mr. Biddle, I take the liberty of informing you of the effects which the alkaline 'pills, recommended by you, have invariably produced upon me; but it is neceſſary to premiſe, that my caſe appears 'to me of a very dubious nature, as both 'the quantity and quality of the ſubſtance 'which I ſometimes void by urine are inſufficient to convince me that my complaint is the gravel. But of that, Sir, 'you may better form an opinion, when I 'mention,

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'mention, that a quantity of wine, by no 'means intemperate, generally produced ſo 'much pain in my kidneys, that to ſtoop, 'or in any manner bend my back, was 'impoſſible; nor could I eaſily turn myſelf 'in bed while this pain laſted, which 'moſtly laſted a week or two, though by 'drinking plentifully of diluting liquors I 'made water without difficulty; yet the 'laſt few drops, and ſometimes the quan-'tity of a tea cup full, were very bloody, 'and came from me with excruciating 'pain; but I never diſcovered any gravel, 'and it appeared to me more like the mu-'cus of the bladder: however, it more 'often happened that I had this fixed and 'intenſe pain in the ſmall of my back, 'without voiding any thing extraneous in 'my urine; and I am ſtill very liable, after 'the uſe of wine, to returns of it; but ſince 'Mr. Biddle has recommended it to me 'to try your pills, I have never failed, by 'uſing them for a day or two, or at fartheſt 'in the courſe of a week, to obtain effec-'tual relief.

'Theſe,

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'These, Sir, are all the particulars of 'my case, which is not important enough 'to merit much attention, nor perhaps 'will it help much to establish the fossil 'alkali as a specific for the gravel, but to 'me it is a valuable discovery, and I have 'no doubt, when it is generally known, 'but it will be beneficial to thousands. I 'beg your pardon for the prolixity of this 'trifling detail, and earnestly desiring that 'you may obtain from others the praise 'and gratitude you have won from me, 'and that all your labours may be crowned 'with success, to the promotion of your 'own fame, and the happiness and im-'provement of your fellow creatures, I 'beg leave, Sir, to subscribe myself,

'Your much obliged and
'very humble servant,

'*Lombard-street*, 'D. LLOYD.
'*June* 26, 1792.

Dr. BEDDOES,
Oxford.

The

The relation of the other facts that have fallen under my own notice would be uſeleſs repetition. All the patients for whom I have directed the foſſil alkali have been paſt, and moſt of them conſiderably paſt, the prime of life. Calculi are ſometimes found in the bladders of children, but the nephritis calculoſa ſeems to be one among the evils almoſt peculiar to declining age. Yet the foſſil alkaliæ, whatever were the infirmities of thoſe who took it, and in ſome inſtances they were very great, has always been perfectly harmleſs, and, but in one caſe, deciſively beneficial in its effects.

The following communications will probably be conſidered as ſtrong additional proofs of its efficacy :

'DEAR SIR,

'I HAD the favour of your letter, and 'am happy to hear it is your intention to 'give the world your ſentiments upon 'the efficacy of ſal ſodæ in calculous diſ-

'eaſes.

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'eaſes. I am ſo well convinced of its 'good effects, that I look upon it as a 'valuable acquiſition. As I write chiefly 'from memory, I ſhall not be able, pro-'bably, to furniſh you with all the caſes 'that have fallen under my obſervation, 'but the few I have recollected you may 'depend upon the accuracy of. The pa-'tient, whom you enquire after, took it but 'a few days, loſt his pain, and has ſince 'had no return.

'Your thoughts upon ſea ſcurvy will 'be intereſting to every one, but par-'ticularly ſo to our ſailors and naval prac-'titioners. I have met with a few caſes '(I think three) in this neighbourhood, but 'they were ſoon cured.

'I am your obedient ſervant,

'J. JONES.

'*Newport*,
'25 *Feb.* 1791.

'CASE I.

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'CASE I.

'JANUARY, 1789, N. N. had been for 'ſeveral years afflicted with gravel, had 'frequently violent fits of pain, and for 'ſome weeks paſt has ſcarcely ever been 'totally free; has with vaſt difficulty voided 'ſeveral large ſtones. He took fifteen 'grains of ſal ſoda, with one of magneſia, 'and cryſtals of tartar, twice a day for a 'fortnight; afterwards about ſeven grains 'of the dry powder of ſal ſoda, with as 'much ſoap formed into pills, for a fortnight 'or three weeks longer regularly. He ſoon 'diſcovered a large mucous and very ropy 'ſediment in his urine, and a great abate-'ment of pain; his fits returned at much 'longer intervals, were ſhorter, and much 'leſs violent, and the ſtones he voided ap-'peared half diſſolved, which before had 'been hard and rough, and when dry 'ſeemed to be covered with a fine ſoft 'powder. In ſix or ſeven weeks he be-'came ſo entirely free from his diſeaſe,

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'that he omitted his medicine for ſome 'months, and has ſince had recourſe to it 'occaſionally only for a fortnight at a 'time. He has never ſince had a ſevere 'fit, but now and then perhaps once in 'three or four months ſome ſlight pain, 'and then voided ſoftened ſtones, more 'like pieces of hard clay than calculous 'concretions.

'CASE II.

'MR. S. a farmer, who had lived 'freely, had been long tormented with the 'frequent paſſing of ſtones from the kid-'neys. I was twice called to him, when 'they were ſo large as to ſtick firmly in 'the urethra, and were removed with con-'ſiderable force by a pair of forceps, con-'trived for the purpoſe, upon the principle 'of Smellie's midwifry forceps.

'He had taken various medicines with-'out any good effect, and upon that ac-'count

'count was with difficulty prevailed upon 'to try the ſal ſodæ. In April, 1789, I 'gave him the dry powder with ſoap (ʒij. 'of each in ſixty pills) of which he took 'two twice a day for near a month, from 'that time he has totally diſcontinued 'them, and when I laſt ſaw him (Oct. '1790) he had never had a return of his 'complaint that he thought worthy of 'attention.

'CASE III.

'In January, 1790, I was called to a 'farmer at ſome diſtance from me, who 'had for ſome years ſuffered much from 'a ſtone in the bladder. He informed me 'that he had taken a variety of what are 'called ſolvents, without the leaſt advan-'tage. He had now been for ſome time 'confined to his room, and had ſcarce 'any reſpite from pain. Under ſuch cir-'cumſtances I ſaw no proſpect of relief, 'but from the operation, which I ſtrongly

'recommended

'recommended to him. He could not 'be prevailed upon to ſubmit to it, 'but was very deſirous of trying any 'medicine I ſhould propoſe. The extra-'ordinary efficacy of the ſal ſodæ I had 'ſeen in the laſt caſe, induced me to try 'its ſolvent powers in this.

'He began upon it immediately, and 'took the uſual doſe twice a day for ſix 'months regularly. In the ſpace of a few 'days his pain abated; in a month he was 'able to walk about his farm; and in the 'courſe of the ſummer walked ſeveral 'times ten or a dozen miles in the day. 'He ſtill continued the pills, but with leſs 'regularity, ſometimes omitting them for 'three or four weeks; and when I laſt ſaw 'him, which was in October, remained 'more comfortable than he had been for 'the laſt two or three years.

'CASE

CASE IV.

'B. a neighbouring farmer, about 75, 'has been for ſome years tormented with a 'ſtone in the bladder. He had now (March, '1790) been for ſome weeks unable to leave 'the houſe, and in frequent and violent pain. 'He took the ſal ſodæ pills; in one week 'his pain was much relieved, and in a ſhort 'time he was able to go about as uſual. He 'ſtill continues much better, and able to 'ride gently, but is ſeldom many days with-'out ſome pain. He ſoon became tired of 'the pills, and in the whole did not take 'them more than a month.'

Dr. BEDDOES, *Oxford.*

In a letter dated June 16, 1792, Mr. Jones informs me, that of theſe patients the third has ſince died of another diſeaſe, and of the fourth, the ſymptoms have ſeveral times recurred, and that with far leſs ſeverity than formerly. He had recourſe to the

medicine a second time, 'but it produced 'such violent heat in his stomach, that he 'could not persevere; he continues however much better than he used to be, 'and able to ride about his farm.'—'I have,' continues my correspondent, 'given the pills in several more cases of 'gravel in the kidnies, and always with 'success.' I have recommended for the last patient the solution with cream of tartar, and, if that should fail, the impregnated alkaline water.

The following letter will be found particularly interesting, as it affords hopes of relieving a very distressing complaint, for which, as for so many others, we have no adequate remedy.

'*Shiffnal, June*, 1792.

'DEAR SIR,

'I AM very glad to hear of your intention to publish your formula, with observations on the soda, but can contribute little to your stock of information upon

'the

'the subject. You are already apprised of 'my opinion of the great use of vegetable 'and fossil alkali in calculous concretions. 'No instances having occurred to me, 'wherein its effects seemed to deserve very 'particular remark, I am unable to offer 'more decisive or more favourable evidence 'of this medicine, than what springs out of 'the general result of my experience. No 'opportunity has yet offered to me of try-'ing its power as a solvent of stone in the 'bladder; but in the nephritis calculosa I 'have had abundant experience of its effi-'cacy, both in promoting the discharge of 'calculi with ease, and often in preventing 'their formation altogether. Yet amongst 'the many favourable proofs which present 'themselves, I must not conceal a few in-'stances, wherein the continued use of this 'remedy has, without any assignable cause, 'failed to procure relief. The proportion 'of these unsuccessful cases is but small, 'and as an additional alleviation, I can ad-'duce some proofs of the utility of the

'ſoda pills in relieving biliary concretions, 'which I know not if I have mentioned to 'you before, and if not, two of them you 'may perhaps think deſerving the trouble 'of a ſhort recital. In both caſes the pa- 'tients were females, and far advanced in 'years. In the firſt inſtance, the diſeaſe had 'exiſted upwards of fifteen years, and re- 'turned pretty regularly at intervals of ſix 'or eight weeks, and ſometimes oftener; 'the paroxyſms occurred with violent pain, 'which uſually laſted many hours; the ſkin 'became deeply tinged with yellow, and 'after a day or two, a ſlight diarrhœa ſeem- 'ed to relieve her. With ſome difficulty I 'perſuaded her to try the remedy, which 'was made into pills with ſoap, and a few 'drops of oleum nunis moſchatæ were 'added. Upwards of three years have now 'elapſed ſince ſhe had any return of the 'ſymptoms. The other lady had ſuffered 'extremely from ſucceſſive ſimilar attacks, 'and was relieved about eighteen months 'ago by the ſame remedy, but is under the

'neceſſity

'neceſſity of a regular perſeverance in the 'uſe of it.

'Believe me, Dear Sir,

&c. &c.

'W. YONGE.'

Dr. BEDDOES.

Several queſtions of great difficulty and ſubtlety may be propoſed reſpecting the operation of alkaline ſubſtances in ſuch caſes as the preceding.—1. Do they merely produce the expulſion of concreted matter? or do they excite ſuch an action of the uropoietic organs, as tends firſt to produce, and then to expel this matter?—I am diſpoſed to adopt the former ſuppoſition; ſince concretions are often lodged in the paſſages without exciting any pain, and ſince I have oftener than once witneſſed the diſcharge of ſmall calculi about the probable period of the beginning operation of the medicine.—Do alkalis act as lithontriptics or otherwiſe? In ſeveral caſes, as that of Bramah, they cannot be eaſily imagined to have any ſuch

operation. The effects are much too ſudden; the ſpeedy and conſiderable diſcharge of gravel makes it probable that they produce, in the pelvis of the kidney or ureters, ſome movements, which obſervation ſhews to be by no means neceſſarily connected with an increaſed ſecretion of urine. When the patient feels this kind of irritation too ſenſibly, the doſe of the medicine muſt be reduced. In ſome inſtances, ſolution ſeems really to have taken place, as in Caſe 1ſt, related by Mr. Jones; and many ſuch are to be found on record. Our ſecretions may, it ſhould ſeem, be altered, either by altering the action of the ſecretory organ, or by preſenting to it materials different from thoſe which it has been accuſtomed to work upon. The urine, by whatever cauſe, is remarkably changed: we are informed on very reſpectable authority, that the urine firſt made after a doſe of mephitic alkaline water, 'will change turnſole paper to a blue 'colour, even if it be not taken above a 'quarter of an hour before the diſcharge of

'the

'the urine *.' I wiſh ſome other teſt of alkalis had been uſed: and ſtill more, that thoſe who have an opportunity of watching the progreſs of patients under a courſe of alkaline medicines would aſcertain—1. Whether the blood undergoes any determinate change.—2. Whether the urine becomes habitually alkaline.—3. Suppoſing this to be the caſe, and the patient to take vegetable alkali, is the exceſs of alkali in the urine, *vegetable*, or foſſil, or volatile? Many other equally curious points of inveſtigation would ariſe in ſuch an enquiry. Of all the ſecretory organs, the kidneys and the mammæ are moſt certainly and quickly affected by the paſſions and by food. By ſtudying the analogy that ſubſiſts between theſe organs, and by experiments not difficult to be contrived or executed, it is probable that we may acquire a conſiderable acceſſion of uſeful knowledge on a ſubject little underſtood.

* Account of the Efficacy, 3d edit. p. 117. Ordinary urine is known to be acid: Mr. Berthollet, the great Pariſian chemiſt, has obſerved the quantity of this acid to be much increaſed by certain diſorders.

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OBSERVATIONS AND CONJECTURES

On the SCURVY,

On OBESITY,

On PHTHISIS PULMONALIS,

AND

On CATARRHS.

IN one or two diſeaſes, the patient is apparently directed by a ſort of inſtinct to the means of cure: in all other inſtances, remedies muſt have originally been the pure bounty of accident. A few facts, however, would ſet ſpeculation to work; and in proportion as the number of ſubſtances capable of producing conſiderable changes upon the living ſyſtem increaſed, the data for analogical reaſoning in medicine were multiplied. It is, in fact, only by ſeizing thoſe new analogies, which are offered from time to time by diſcoveries in the phyſical ſciences, that

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that we can hope to improve the art of medicine. Nor will any one, who will take the pains to comprehend this ſimple truth, require an apology for attempts to form new combinations of this ſort.

Mayow not only diſcovered ſeveral elaſtic fluids, and the eſſential properties of the moſt active of them all, but he aſpired to change the whole face of medicine and phyſiology, by the application of his wonderful diſcoveries to the appearances of animal nature. Immediately upon the revival of this branch of chemiſtry, Dr. Macbride attempted to derive the ſame advantage from it, and the minds of ſucceeding philoſophers have been engaged by ſimilar ſpeculations.

The two annexed papers of Dr. Girtanner afford an extremely ingenious ſpecimen of ſuch ſpeculations. His experiments are ſeveral of them happily imagined; that by which he has at once ſhewn the falſehood of the concluſion which Fontana had drawn from his laborious experiments on poiſons,

is

is conceived in a ſpirit far more philoſophical than that which has directed the greater number of experiments upon animals. His reaſonings are an attempt to inveſtigate the laws of organic bodies, by combining the medical opinions of Dr. Brown with Mr. Lavoiſier's theory of chemiſtry. He borrows little from Haller, beſides the term *irritability*, and if he has not mentioned the name of Brown, he has made a free uſe of his doctrines, and often employed his very expreſſions. He might not, perhaps, in an abſtract, think it neceſſary to point out the ſource from which ſo many of his ideas are derived, but in his larger work we have a right to expect that he ſhould do juſtice to departed genius.

The *Journal de Phyſique*, from which theſe papers are taken, is indeed abundantly known to the cultivators of ſcience, but it can by no means be ſuppoſed to fall into the hands of the majority of the practitioners of medicine in this iſland; and I was very willing to enhance the value of this little publication

publication by annexing to it a train of reflections, in general well worthy of their notice, and calculated to excite their reflections. Mr. Woodhouſe, of the Middle Temple, at my requeſt, dedicated ſome hours of leiſure at Oxford to the tranſlation, and his knowledge of the French, and of his own, language, would probably have concealed from the reader that he has not made phyſiology his peculiar ſtudy.

For ſeveral years paſt I had been attempting to diſcover ſome part of the effects of oxygene air upon the animal œconomy: it appeared likely that its abundance or deficiency would ſenſibly affect the health, and that the chemical compoſition of the fluids and ſolids of the living body would influence their properties not leſs than the properties of dead matter, though not perhaps exactly in the ſame way. In ſome inſtances I thought I perceived as much certainty as either could be expected, or as is any where to be found in medical reaſonings, and in others there appeared a faint glimmering of probability,

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probability, where total darkneſs has hitherto prevailed. The ſcurvy (ſea ſcurvy) I have long conſidered as offering an application of the pneumatic chemiſtry, nearly as direct and beautiful as the phænomena of reſpiration; and it would be eaſy to prove, by the teſtimony of different perſons, that I had long ſuppoſed this diſeaſe to be owing to a gradual abſtraction of oxygene from the whole ſyſtem, juſt as death is produced in drowning, by withholding all at once the ſame ſubſtance from that blood which is to paſs to the poſterior cavities of the heart. The proofs of this theory ſeemed equally ſimple and ſtrong; the livid colour of the blood, and the large livid ſpots which are ſo often ſpread over the ſurface of the body, left little room to doubt of the abſence of oxygene; and the recovery of the ſick, by the adminiſtration of acids, and by a vegetable diet, afford a ſort of confirmation ſimilar to that which is derived from chemical ſyntheſis, for no ſubſtances are better calculated than acids at leaſt, to impart oxygene to the ſyſtem; they contain it in abundance, and they eaſily part with it.

There

There must frequently be observed by those who attend to the effect of evidence upon different minds, a species of intellectual cowardice, which refuses its assent to just evidence, as long as a single difficulty remains, though the facts constituting this difficulty do not oppose, any more than they coincide with, the theory. In hopes of clearing up such difficulties, and applying the hypothesis to the principal symptoms, I from time to time deferred the publication of an opinion, in support of which I had collected various proofs.

In the mean time, the same theory occurred to a physician, whose acquisitions and powers of reflection do him the more honour, as the greater part of his life seems to have been spent amid the hurry and incommodiousness of a seafaring life *. Such an anticipation is very natural in the present

* *Observations on the Scurvy. By Thomas Trotter, M.D.* 1792. In the first edition of this ingenious treatise no such theory is hinted at. The first edition was published in 1786.

period of ſcience, and however mortifying it may be to vanity, it affords the moſt powerful encouragement to perſevere in attempts to aſcertain the principles of the moſt important and moſt imperfect of all the arts, inaſmuch as it proves the poſſibility of reaſoning ſucceſsfully concerning a diſeaſe, which you ſcarce know but by deſcription.

Dr. Trotter, (from page 125 to 150) ſtates the leading circumſtance in this diſeaſe, the privation or diminiſhed proportion of oxygene in the blood. He quotes ſome of the fine experiments of Dr. Goodwyn, and mentions the exiſtence of oxygene in acids, which, he juſtly imagines, they reſtore to the blood. He does not enter into any other particulars of theory. 'The *ratio* '*ſymptomatum*,' he modeſtly obſerves, 'is 'certainly a difficult ſubject to enter upon. 'In what manner a diſeaſed ſtate of the 'blood communicates its influence to the 'moving powers of the body we are at a 'loſs to explain.'

It

It may be expected of a theory framed by two perſons independently of each other, that the ſhades ſhould differ, although the outlines be the ſame. My reflections on the ſcurvy have been more minute, and, in many reſpects, my concluſions vary from thoſe of this experienced writer. I ſhall leave to the reader's conſideration the two leading proofs ſtated above, and ſo largely expatiated upon by Dr. Trotter, and add ſome miſcellaneous obſervations, which may tend ſtill farther to eſtabliſh the theory. I ſhall alſo frankly intermix my objections to ſome of his opinions, chearfully ſubmitting it to his ample experience to decide between us, in a full expectation that his regard to truth and utility will abſorb all perſonal conſiderations.

1. In the firſt place, it is not quite accurate to impute the diſeaſe to a deficiency of oxygene in the blood. The deficiency is, doubtleſs, general to the whole ſyſtem. The diſcolouration of thoſe of the ſolids, of which the colour evidently depends upon oxygene,

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oxygene diſtributed to them from the blood, beſpeaks a deficiency here alſo. This diſcoloration is noticed in moſt of the accounts we have of ſcurvy. 'The gums 'have an unuſual livid appearance,' ſays Lind (3d edition. p. 100). In one paſſage the heart is ſaid to be white and putrid, the lungs blackiſh and putrid (p. 240); in another the muſcles are deſcribed as mortified (p. 242), and as falling to pieces on being handled. Dr. Blane could not diſcover, upon diſſection, any effuſion of blood (*), where the livid ſpots appeared. Hence too it may be concluded, that the hardneſs and ſtiffneſs of the muſcles and tendons, a ſymptom generally obſervable in ſcurvy, depend upon the abſence of oxygene.

As oxygene is neceſſary to the contraction of the muſcles, it is probable that it is conſumed, or, more properly ſpeaking, enters into ſome new combination, in

(*) Obſervations on the Diſeaſes of Seamen, 1789, p. 515.

conſequence of which it is eliminated out of the body; for we cannot but ſuppoſe that the quantity employed correſponds to the vaſt quantity imbibed. Hence we may underſtand the final cauſe of quickened reſpiration, during great exertions of the muſcles. This ſeems alſo to explain an obſervation, which has ſo frequently been made at ſea, that the ſcurvy makes its appearance after a ſtorm, when the ſeamen, having undergone violent exerciſe, have expended a great part of the oxygene of the ſolids.

2. One of the moſt pleaſing, and, if I conceive it juſtly, of the ſtrongeſt poſſible arguments, may be drawn from ſome appearances in the thorax after death. Dr. Goodwyn found, that in ſuffocated animals the left cavities of the heart are full of venous blood. I have had abundant opportunities of verifying the truth of this obſervation. The death of ſcorbutic patients frequently ſeems to enſue, in conſequence of the blood being ſo deſtitute of oxygene, as to be incapable

capable of ſtimulating the left auricle and ventricle of the heart: perhaps alſo the inirritability of the muſcular fibres of the heart itſelf may contribute to this effect. The following paſſages will eſtabliſh the fact: 'The 'cavities of the heart were quite full of *cor-* 'rupted blood.' Lind. p. 240. 'All thoſe 'who died ſuddenly, without any viſible 'cauſe of their death, had,' according to another account, 'the auricles of their 'heart as big as one's fiſt, and full of coa- 'gulated blood.' p. 241.

Dr. Lind, from his own diſſections, mentions, that 'in patients, whoſe deaths were 'unexpected and ſudden, and where no 'effuſion of blood could be perceived into 'any cavity of the body, the heart was 'commonly much diſtended with blood; 'the auricles and ventricles of both ſides 'were filled, but thoſe on the right in the 'greateſt degree.' p. 503. This phænomenon would engage Dr. Lind's attention the more, as he was aware that though the right ventricle after death is generally diſ-

tended with blood, 'the left ſeldom con-
'tains any.' p. 503.

This want of ſtimulating power in the blood, and of irritability in the heart, will perhaps account for the ſymptoms ſo often mentioned, anxiety, tightneſs of the breaſt, difficulty of breathing.

3. Dr. Trotter conſiders, 'a deficiency 'of *recent* vegetable matter *alone*, as the 'occaſional cauſe of the ſcurvy.' p. 171, 172. Yet we are certain that the blood, in the firſt inſtance, and afterwards the ſolids, are oxygenated by means of the lungs. They may acquire this principle by means of the ſtomach; but we have no direct experience of their doing ſo. It is only an inference from the compoſition of acids and vegetables, and from their effects in the ſcurvy. Between the reception of any given food into the ſtomach, and the oxygenation of the blood by that food, there muſt intervene unknown proceſſes. It appeared therefore probable to me, that as ſeamen

ſeamen in general breathe an air containing a ſmaller proportion of oxygene than other deſcription of perſons, the ſcurvy might often originate from this cauſe, and may be prevented or cured by guarding againſt it; for whether we oxygenate the blood by the lungs or the ſtomach, a diſeaſe depending on the want of this principle may, one would ſuppoſe, be equally obviated; nor would it be eaſy in the preſent ſtate of our knowledge to aſſign any circumſtance that diſtinguiſhes the two caſes, except the heat ſupplied by oxygene, when preſented in the ſtate of an elaſtic fluid, though it would be raſh to deny that other differences may exiſt. Captain Cook's unexampled ſucceſs in preſerving his crews from the ſcurvy during his two laſt voyages, ſeems in great meaſure owing to his extreme care to keep his ſhips well aired. On many occaſions they were reduced to ſalt proviſions, and much longer out of ſight of land, than many other ſhips, which have been dreadfully afflicted with the ſcurvy; in his laſt voyage there

never appeared among his crew any ſymptom of this diſorder; and in his ſecond only one man had it in any conſiderable degree.

4. It is extremely difficult to find ſuch preciſe facts as ſhall amount to an *experimentum crucis*, eſpecially as obſervation has not yet been guided by this theory. But Dr. Trotter has himſelf furniſhed an important obſervation, from which, if any one were to decide between theſe two cauſes of ſcurvy, want of freſh vegetables, or want of air ſufficiently furniſhed with oxygene, he muſt, I think, decide without heſitation in favour of the latter; and here I appeal to him againſt himſelf. In July 1783, a ſhip, of which he was ſurgeon, arrived at Cape La How, on the Gold Coaſt of Africa. In the ſpace of a week above 'an hundred prime 'ſlaves, young, ſtout, and healthy,' were purchaſed. The competition however of the purchaſers at Anamaboe, whither this ſhip afterwards ſailed, ran ſo high, that in February, 1784, it had not on board two thirds

thirds of its complement. An indiſpoſition now began to prevail among the ſlaves, which ſoon afterwards proved to be the ſcurvy; and before the arrival of the veſſel at Antigua, of near ſix hundred and fifty, near fifty had died, and about three hundred were tainted, in different degrees, with the ſcurvy. Before they quitted the coaſt, ſeven or eight had died, and between ſeventy and eighty were ill.

1. Of theſe ſlaves, 'the food conſiſted 'of beans, which were brought from Eng- 'land, and rice and Indian corn, which 'were bought on the coaſt. Theſe arti- 'cles were boiled to the conſiſtence of a 'ſoft paſte, and made as near as poſſible 'like the food of the country, by the ad- 'dition of palm oil, Guinea pepper, and 'common ſalt—they were allowed to drink 'what water they pleaſed.' p. 52.

2. They were 'confined below ſixteen 'hours out of twenty-four, and permitted 'no exerciſe when upon deck.' (ibid.) 'The

'rooms, where they are ſecured below, are 'from five to ſix feet in height. They 'are ſtowed *ſpoonways*, and ſo cloſely 'locked into one another's arms, that it is 'difficult to move without treading upon 'them. The rooms are imperfectly aired 'by gratings above, and ſmall ſcuttles in 'the ſide of the ſhip, which, of courſe, 'can be of little uſe at ſea. The gratings 'are alſo half covered when it blows hard, 'to keep out the ſalt ſpray or rain. The 'temperature in thoſe rooms, when they 'became crowded, was above 96° of Fah-'renheit's ſcale.—I myſelf could never 'breathe there, unleſs under the hatchway. 'In ſuch ſituations it may be ſuppoſed, 'that the ſufferings of theſe creatures are 'ſometimes dreadful. Air, heated and 'rarified to ſuch a degree, and loaded with 'animal effluvia, cannot fail of being nox-'ious to life. There were certainly in-'ſtances where ſome expired from ſuffo-'cation, having ſhewn no previous ſign 'of indiſpoſition.' p. 54, 55.

3. With

3. With theſe two facts, let the reader compare the following: 'Few of the 'boys had any ſcorbutic ſymptoms; none 'of them were ſhackled; and by being 'allowed to run about the deck, and occa'ſionally aſſiſt in the duty of the ſhip, 'their health ſeemed to be preſerved by 'the exerciſe. This was alſo the caſe 'with the women, for out of the whole 'number eight only were affected.' (p. 63.) 'During this ſickly ſtate of the ſhip, none 'of the ſailors were in the leaſt tainted 'with the ſcurvy. Their diet was the 'common ſea fare; a little of the victuals 'prepared for the ſlaves was generally eat 'with the ſalt beef; *they had it however in 'their power* * to barter ſome of their pro'viſions with the natives for freſh vegeta'bles.' p. 64.

The following is the inference which the author draws from theſe facts: 'I am ex'tremely unwilling to admit the conta-

* Did they all make uſe of the opportunity?

'gious

'gious nature of this diſeaſe; and if it 'cannot be propagated that way, it is 'moſt likely that the tainted atmoſphere 'of the ſlave rooms, which were now full, 'ſo powerfully prediſpoſed thoſe late pur-'chaſed negroes to ſcurvy, that the ex-'citing cauſe was much accelerated in its 'operations, by the foul air which they 'breathed; impure exhalations have there-'fore been deſervedly mentioned by au-'thors among the *remote* cauſes of the 'ſcurvy.' p. 68. For my part, I ſhould preſume that the diet of theſe unhappy people could have no ſhare whatever in producing the diſeaſe, which delivered ſo many among the men from their preſent and impending calamities.

5. There are other facts which ſeem to ſhew that too much is attributed by Dr. Trotter to *freſh* vegetables. Linnæus informs us, that the Laplanders are unacquainted with the ſcurvy; they feed all the winter on the freſh fleſh of the reindeer. 'This exemption of the Laplanders 'from

'from the general diſtemper of the north,' ſays Pringle, 'is the more obſervable, as 'they ſeldom taſte vegetables, bread never.' (Cook's Voyages, from 1772 to 1775. vol. ii. p. 376.)

Conſidering freſh meat, or the muſcular part of animals, chemically, I ſee no reaſon why it ſhould not be efficacious in preventing or curing the ſcurvy. Oxygene it contains, when *raw*, in a ſtate of looſe combination, though probably not in ſuch large proportion as vegetable ſubſtances, even ſuch as are not acid. I had noticed in travellers of great reſpectability paſſages that confirm this idea. The nations inhabiting the cold and dreary regions on the eaſtern ſhores of Aſia, and the oppoſite coaſts of America, ſeem to have learned from experience, that freſh, or at leaſt unſalted fiſh is a preventative of the ſcurvy, or a remedy for it. Thus Dr. Pallas (Reiſe, iii. 47.) deſcribes the Oſtiack Tartars of the Oby, as preparing their winter ſtores altogether

ther without ſalt. 'They are extremely apt, 'when diſabled by age or infirmities, to be-'come ſcorbutic. In winter they ravenouſly 'devour their frozen fiſh *raw*, a practice 'which the neighbouring Ruſſians imitate, 'eſteeming them a preſervative againſt the 'ſcurvy.' (46.) Mr. Meares (Voyage, Introd. p. 30.) ſpeaking of an American tribe, ſays, 'She made us ſenſible that the ſame diſ-'order (ſcurvy) prevailed in her nation; 'and that whenever the ſymptoms ap-'peared, they removed to the ſouthward, 'where the climate was more genial, and 'where plenty of fiſh was to be obtained, 'which never failed to prove the means 'of their recovery.'

The reader will probably agree to conſi-der the frozen as freſh fiſh. If it were poſſible to preſerve meat on ſhip board, in this ſimple manner, one great ſource of the ſcurvy would probably be cut off. Cookery combines the oxygene anew; would our ſailors eat *raw* animal food?

Dr.

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Dr. Lind, though he has full confidence in green vegetables, and affirms that the ſcurvy never can 'become a general, fatal, and deſtructive calamity,' where they abound, and the proper method of treatment is known and purſued (p. 541.); he concludes from a number of comparative trials (p. 538), that certain patients in Haſlar hoſpital in general grew better, notwithſtanding they abſtained altogether from vegetables. 'This ſtrict abſtinence from the 'fruits of the earth,' ſays he, 'was continued long enough to convince me, that 'the diſeaſe would often, from various circumſtances, take a favourable turn, independent of any diet, medicine, or regimen.' We have nothing, I preſume, to oppoſe in point of concluſiveneſs to ſuch experiments made by a phyſician ſo intelligent and ſo experienced in this particular diſeaſe.

The following caſe, for which we are indebted to Dr. Sandifort, Profeſſor of Anatomy at Leyden, is among the moſt extraordinary

extraordinary in the records of medicine. Its value however exceeds its ſingularity. Dr. Goodwyn quotes it in ſupport of his doctrine of reſpiration. It no leſs corroborates the foregoing theory. After the local ſymptoms ariſing from the conformation of the heart are ſet aſide, there will remain the principal characteriſtics of ſcurvy, livid ſpots or blotches, a bloated countenance, hæmorrhages, exceſſive laſſitude, diſtreſſing anxiety, frequent faintings upon ſlight motion, and a very offenſive breath, without an impaired appetite, and with a tendency to ſalivation. Diſtinct as the account is, I wiſh for ſtill greater minuteneſs, in a firm perſuaſion that every additional circumſtance, by affording a new analogy, would tend ſtill further to ſhew that the ſymptoms of ſcurvy ariſe from a deficient ſupply of oxygene to the blood by way of the lungs. For nearly the firſt year of the life of the infant in queſtion, who was born November 17th, 1764, there was no appearance of diſeaſe. The parents were healthy; the child was put out to nurſe;

he grew apace; 'but was ſcarce a year 'old *, when thoſe dreadful ſymptoms, 'which ſo much harraſſed him during the 'ſubſequent portion of his life, manifeſted 'themſelves. The livid colour of his 'nails and fingers firſt drew the attention 'of the parents. This hue was not con-'ſtant; it was not occaſioned by any 'tightneſs of dreſs; it did not at firſt ap-'pear of ſuch conſequence, as to induce 'them to call in a phyſician, eſpecially 'as the child ſeemed in other reſpects 'healthy, and made ſuch progreſs as to be 'able, by the end of the ſecond year, to 'walk alone.

'He now began to complain of great 'laſſitude upon the leaſt exertion. A 'catarrh, accompanied with a violent op-'preſſive cough came on. The child ob-'ſtinately refuſed the medicines preſcribed 'by the phyſician, who was now called 'in, and the next day a number of ſpots,

* Obſervationes Anatomico-Pathologicæ, Lugd. Batav. 1777. p. 11, & ſeq.

'which

'which at firſt were of a red colour, and 'then turned livid, were obſerved upon 'the face. The cough continued troubleſome; motion became exceedingly diſagreeable; and after any fatigue, the 'face, hands, and feet appeared remarkably livid; the tongue and lips were 'nearly black, but the natural colour returned, upon remaining quiet ſome time. 'Theſe alternations were viſible almoſt 'every day. Meanwhile the child grew 'rapidly; the appetite was very good, and 'there was no complaint, but of laſſitude, 'preſſure on the top of the head, anxiety, 'eſpecially in winter, and ſuch chillineſs, 'even internal, that in winter he could 'not keep himſelf warm by the fire-ſide; 'nor did the rays of the ſun, on the hotteſt 'days, produce their natural effect, much 'leſs occaſion any ſweating.

'March, 1767. Bleeding ſeemed to 'leſſen the anxiety and ſenſe of preſſure 'for a time. The blood was thick and 'black, and no craſſamentum ſeparated.

'Towards

'Towards the close of this year he had 'the small-pox mildly, and without any 'aggravation of his anxiety; nor did the 'measles and chicken-pox, which he 'caught a few months afterwards, produce 'any change either for the better or the 'worse. The symptoms above mentioned 'attacked him the moment he moved, 'especially the anxiety, which was accom-'panied with such violent palpitation, that 'the strokes of the heart could be seen, 'and even heard. Riding, which was now 'recommended, proved of no service, nor 'could he bear it long at a time.'

Gaubius, being consulted in 1769, advised cold bathing and rubbing the body, but to no purpose; bleeding, and gentle motion were equally ineffectual; the anxiety, when extreme, was attended with a dry cough. 'The breath had an un-'common smell, much resembling that 'of an egg opened immediately after 'boiling.

'This diſtreſſing ſituation continued till '1774, by which time the anxiety and 'violence of the palpitations were much 'increaſed, and faintings, pain, like preſ-'ſure, in the head, a ſwelling and pulſation 'of the jugular veins, were obſerved. 'Riding being utterly impoſſible, frequent 'airings in a carriage were ſubſtituted in 'its ſtead. In May bleeding diminiſhed 'the anxiety; and he could now walk 'about for an hour, without any great fa-'tigue, and his parents began to entertain 'hopes of his recovery. But all the ſymp-'toms recurred with greater violence in 'autumn; he coughed violently, and ſpit 'up, ſometimes mucus ſtreaked with blood, 'ſometimes pure blood. For this he was 'twice bled in November, within four 'days, but the anxiety ſtill continued, and 'ſometimes roſe to ſuch a pitch, as to 'threaten immediate death. The child 'was ſenſible that he ſhould not long ſur-'vive; he often remarked that his diſorder 'was quite unknown and incurable, and 'that

'that no perſon could conceive what he 'felt about the heart. Another bleeding 'in December afforded a tranſitory relief; 'but the ſetting in of winter, a ſeaſon al-'ways intolerable to him, excited the ut-'moſt alarm in the parents.'

During the following year, the complaints continued equally diſtreſſing. In 1776, 'all motion became impoſſible; on 'the ſlighteſt exerciſe, he would faint, 'diſcharge a great quantity of ſaliva from 'his mouth, and continue blind for a time; 'all attempts to relieve him were vain; 'the anxiety was much increaſed, eſpe-'cially on lying down; what had formerly 'amuſed him now became indifferent; his 'face was bloated; his feet became œdema-'tous; yet his life was protracted in miſery to 'the 8th of March, 1777, when, upon being 'ſeized with exceſſive anxiety, he died.'

Dr. Hahn, who attended him during the laſt year, communicated to Dr. S. the following particulars:

'Immediately upon the very firſt glance, I 'recognized the complaint of which I had 'heard ſo much, and which report, as 'uſual, ſeemed to have exaggerated: the 'child was aſthmatical; on the ſlighteſt 'motion he breathed with ſuch difficulty 'that his face and hands became as livid 'as in a ſtrangled perſon; ſometimes they 'looked as if painted blue.

'The cauſe of ſo ſevere and ſo long-'continued an aſthma was obſcure; nor 'did it appear certain when the diſeaſe 'began. The parents, and the phyſician 'who had hitherto attended him, agreed 'in aſſuring me, that he was born healthy, 'and that no ſigns of complaint in the 'cheſt had occurred during the firſt year; 'nor was it till the ſecond year that the 'blue colour and ſymptoms of aſthma had 'been obſerved; even then the complaint 'was not conſtant, but became worſe and 'worſe, as the patient advanced in years 'and ſize.

'The

'The child ſeemed to me handſome, well-made, and tall for his years. He had a conſtant difficulty of breathing, which increaſed in cold weather, and upon motion; the face was bloated; the eyes were protuberant, fixed, and betrayed uneaſineſs. While the aſthmatic fit continued, his face appeared as it uſually does in a perſon who has been long walking apace againſt the wind in winter; the cheeks, point of the noſe, the ears, as well as hands, fingers, and nails became livid; the lips, tongue, and inſide of the mouth were of a deep purple; the pulſation of the carotids was viſible at a diſtance; the pulſe at the wriſt very fluctuating.

'The child was ſenſible; his temper variable; but he was commonly moroſe and peeviſh; during his ſhort intervals of chearfulneſs, you could diſcover in his eyes and on his forehead, even while he ſmiled, a latent ſenſe of ſuffering; different complaints were perpetually re-

'curring, as head-ach along the ſagittal 'ſuture, ear-ach, pain in the breaſt and 'left hypochondriac region, of ſickneſs, 'pain in the belly, extending as low as the 'os pubis, chillineſs, &c.

'At different times his ſufferings varied 'conſiderably. The following circum-'ſtances I conſtantly obſerved:

'1. Great dyſpnœa upon motion; 'viſible pulſations in the neck.

'2. A face too full for the habit of the 'reſt of the body; during his laborious 'reſpiration a livid colour of the coun-'tenance, a protuberance, and occaſional 'ſuffuſion of the eyes.

'3. Urine always high-coloured, with-'out ſediment.

'4. Great coſtiveneſs.

'5. Conſtant chillineſs, even though 'the

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'the ſkin felt warm; this ſenſation never 'left him, except after he had become 'quite warm in bed. In winter, though 'ſitting cloſe by the fire, he complained of 'ſhivering, and in ſummer he longed for 'a large kitchen fire, and deſired to baſk 'in the ſun during the hotteſt part of the 'day.

'6. He was ſometimes much relieved 'by opening medicines, and by hæmor-'rhages from his noſe, which happened 'from time to time.

'The tongue was very foul; the breath 'extremely offenſive *(fœtidiſſimus)*.'

After quoting ſome caſes, not, I think, altogether in point, Dr. S. gives the following account of the diſſection. The thorax alone was opened.

'The pericardium did not, as uſual, 'appear ſurrounded by the lungs, and al-'moſt incloſed in them; but a maſs was

'ſeen to fill nearly the whole cavity, and 'to compreſs the lungs extremely: this 'maſs was the pericardium, containing the 'heart in a ſtate of great diſtention, and 'very full of blood. It reached from the 'diaphragm (which on the right ſide roſe 'to the fifth, on the left only to the ſixth 'rib) to the ſpace between the firſt and 'ſecond rib, and ſo entirely filled the late-'ral parts of the thorax, that only the an-'terior portion of the lungs on the right 'ſide (viz. the margin of the ſuperior 'and middle lobe), and but a very ſmall 'portion on the left (viz. towards the 'upper and lateral part) could be ſeen. 'Above the pericardium, the ſuperior 'cava, with the origin of the ſubcla-'vian veins, appeared turgid with black 'blood.

'Upon opening the pericardium, ſome 'water ran out; but not more, indeed not 'ſo much, as is ſometimes found in ſub-'jects where no dropſical ſymptoms have 'preceded death.

'The

'The heart, when freed from its ſack, 'appeared exceſſively turgid, not however 'equably ſo: both the ventricles were not 'diſtended to the ſame degree; the right 'ventricle, as well as its auricle and ſinus, 'were much more enlarged, and full of 'blood, than the left; all the veins, which 'ramify from the coronary veins along the 'ſurface of the heart, were ſo dilated even 'to their extreme branches, that the moſt 'ſucceſsful injection could not have ren-'dered them more diſtinct.

'The veins ariſing from the ſubclavian, 'more eſpecially the jugular, were enor-'mouſly diſtended with thin black blood; 'the ſuperior vena cava, where it is 'lodged within the pericardium, did not 'much exceed its natural ſize; the inferior 'cava was enlarged; the pulmonary veins 'were turgid, but not exceedingly ſo; 'the aorta was enlarged at its origin; the 'pulmonary artery was remarkably con-'tracted, from its origin almoſt to its bi-'furcation; of the arterious duct, or rather

'ligament,

'ligament, as it would have been at this 'age, there was no veſtige.

'The lungs externally had no morbid 'appearance; but they were ſmall, com-'preſſed, and not eaſily dilatable; whence 'it appeared that they could not properly 'have performed their functions.

'The external appearance of the heart 'ſhewed where the ſource of the miſchief 'lay. After tying up all the veſſels, it 'was ſubmitted to further examination.

'The right ſinus and auricle were firſt 'opened; a large quantity of thin black 'blood flowed out. In the foramen ovale 'there was an aperture, which would ad-'mit a large probe.

'On introducing the finger into the right 'ventricle, and turning the point towards 'the orifice of the pulmonary artery, where 'it uſually ariſes from this ventricle, no 'ſuch orifice could be felt, but it eaſily

'ſlided into another, and that a very large 'one. But how great was my aſtoniſh-'ment, and that of all the bye-ſtanders, 'when it was diſcovered, that the finger 'had paſſed into the aorta, which, accord-'ing to the ordinary law of nature, has 'no communication whatever with the 'right ventricle.

'This ventricle was divided in the 'place oppoſite to the valve, behind which 'the arterious orifice lies, quite down to 'the apex. Upon lifting this valve a lit-'tle, the large mouth of the aorta ap-'peared, as alſo a ſmooth margin; beyond 'which the finger found a way into the 'left ventricle of the heart. Upon cutting 'the aorta tranſverſely, at a proper diſ-'tance from the ſemilunar valves, the 'ſame margin was ſeen to divide its orifice 'into two parts, the larger communicating 'with the right, the ſmaller with the left 'ventricle.

'The aorta therefore aroſe from both 'ventricles,

'ventricles, and muſt have received all the 'blood from both.

'The pulmonary artery having been 'cut acroſs above the valves, they ap-'peared very ſmall, almoſt grown to the 'artery, and covered with a granular ſub-'ſtance, reſembling a fleſhy excreſcence, 'ſo that only area enough was left to 'ſuffer a ſmall probe to paſs into the ven-'tricle, and even this paſſed with greater 'difficulty from the ventricle into the 'artery. Upon opening the orifice longi-'tudinally, we found only two ſhapeleſs 'valves, partly covered with the ſame gra-'nular excreſcence.

'In the mouth of the aorta there were 'three valves; in the left ventricle there 'was nothing remarkable, except the aper-'ture in the ſeptum, and the thinneſs of 'its ſubſtance, which did not exceed, and 'indeed ſcarce equalled, that of the right 'ventricle.'

From

From the ſeveral particulars of this wonderful hiſtory and diſſection, the reader will be able to draw ſome important concluſions, beſides that already pointed out. 1. The comparative capacity and ſtrength of the ventricles in this caſe confirms Dr. Goodwin's diſcovery of the inirritability of the left ventricle by venous blood. Here the right ventricle ſeems to have performed almoſt all the labour of circulation: hence its enlargement, its equal or ſuperior thickneſs, the left not having, in conſequence of action, outſtripped it in this reſpect, as it does in healthy perſons; hence, probably, alſo, the larger opening of the aorta into the right ventricle. 2. This caſe removes all doubt as to the neceſſity of oxygene to the due action of the muſcles; a fact which the penetrating genius of Mayow perceived, though he miſconceived the mechaniſm of muſcular action, which we do not yet underſtand. 3. The Dutch phyſicians ſeem to have been ſomewhat at a loſs to account for the health enjoyed by the infant during the firſt year. But the full

full expanſion of the lungs is not neceſſary to life or health in early infancy, during which, for a ſhorter or longer period in different ſubjects, part of the blood paſſes through the foramen ovale and arterious duct, and is therefore not fully oxygenated *. Dr. Sandifort is doubtleſs right in concluding, that the ſtructure was connate; no rupture of the heart could have produced it. His information that the child ſcarce cried, never coughed, and was extremely quiet during the firſt year, deſerves notice. 4. From what has been ſaid above, the peculiar laſſitude felt on very high mountains, and deſcribed by M. Sauſſure † from

* Hence, probably, the *ſublivid* infantile complexion, which diſappears at various ages, juſt as the remains of the fœtal circulation ceaſe, and is perfectly diſtinct from the occaſional jaundice of infants.

† 'Les forces muſculaires s'epuiſent avec une extrême promptitude.—Ce qui diſtingue & caracteriſe le genre de fatigue que l'on eprouve á ces grandes hauteurs, c'eſt un epuiſement total, une impuiſſance abſolue de continuer ſa marche——On ne feroit pas á la lettre quatre pas de plus, fût-ce pour eviter le danger le plus

from his own ſenſations, may perhaps be ſatisfactorily explained. In this ſituation fatigue

‘ plus eminent.——Si l'on perſiſte a faire des efforts, on ‘ eſt ſaiſi par des palpitations & par des battemens ſi ‘ rapides & ſi forts, dans toutes les arteres, que l'on ‘ tomberoit en defaillance ; ſi on l'augmentoit encore ‘ en continuant de monter.

‘ La ſeule ceſſation de mouvement, dans trois ou qua- ‘ tre minutes, ſemble reſtaurer ſi parfaitement les forces, ‘ qu'en ſe remettant en marche, on eſt perſuadé qu'on ‘ montera tout d'une haleine juſques à la cime de la ‘ montagne. Or dans la plaine une fatigue auſſi grande ‘ ne ſe diſſipe point avec une telle facilité. Mr. ‘ Pictet ſe trouve toujours ſaiſi d'une eſpece d'angoiſſe ‘ (anxiety), d'un leger mal de cœur, & d'un degout ‘ abſolu, deſqu'il eſt arrivé a la hauteur d'environ 1400 ‘ toiſes au deſſus de la mer.’ (Sauſſure Voyages, 4to. I. 482, &c.)

The profound ſleep (*aſſoupiſſement, ſommeil preſque lethargique*) ſeems exactly the aſphyxia ariſing from an improper air. The laſſitude is only the firſt degree of this aſphyxia. The anxiety, palpitations, &c. are all kindred ſymptoms, and what almoſt demonſtrates that they are here imputed to the right cauſe is a ſubſequent obſervation of Mr. Sauſſure, who tells us, that on the Col de Géant, at 1763 toiſes above the level of the ſea, ‘ Le charbon ne brûloit que d'une maniere languiſſante,

‘ &a

fatigue very ſuddenly comes on, and as ſuddenly goes off on reſt. Now in aſcending theſe rugged heights, the muſcular exertion muſt expend a great deal of oxygene, which the rarified atmoſphere will ſupply but ſcantily. Hence the neceſſity of ſuſpending the ſevere exerciſe to collect a ſtock of this principle, and hence we ſee why it is ſo ſoon exhauſted. The other ac-

'& a force d'être animé par le ſoufflet.' (*Journ. de Phyſ. Sept.* 1788, *p.* 209.)

Among the deſiderata in medicine, few are, I think, more felt than greater choice in the means of procuring ſleep. Opium, and other diffuſible ſtimuli, in ſome caſes, increaſe the reſtleſſneſs; and in moſt caſes it would be better to induce ſleep by the abſtraction of ſtimuli, than by exhauſting the excitability. Upon this principle we could not have a better ſoporific than an atmoſphere with a diminiſhed proportion of oxygene air: ordinary air might be admitted, when the patient was once laid aſleep. Mr. Sauſſure repeatedly mentions the very ſound ſleep he enjoyed during the nights he ſpent in the high ſituation above mentioned. The cold of which people traverſing great heights complain, I partly aſcribe to the want of oxygene to ſupport animal heat. From the narratives I collect that the ſenſible cold is greater than the thermometrical.

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cidents of reſpiration, which have been aſcribed to a loſs of equilibrium between the external and internal air, probably depend upon the ſame cauſe. Mr. Plantade, at the age of ſeventy, ſuddenly and placidly expired, beſide his quadrant, upon the heights of the Pyrenees. Had he been opened, the left cavities of the heart would, I ſuppoſe, have been found turgid with blood, and, had he been carefully obſerved, his countenance would have ſhewn ſigns of ſtrangulation. He died, in ſhort, I conceive, nearly as he would have done in the exhauſted receiver of an air-pump, with this difference, that the preceding fatigue had conſumed the irritability of his muſcles. Young animals bear the abſtraction of ſtimuli better than old ones; and there is, probably, a certain mixture of oxygene and azotic airs that would be fatal to old, but not to young perſons. The experiments of Mr. Sauſſure, Pini, and Reboul, concur in ſhewing, that, independently of its rarefaction, the atmoſphere of very elevated mountains contains a far

ſmaller proportion of oxygene than that of the lower regions, eſpecially than that of the high vallies of the Alps.

7. Therefore the inhabitants of theſe great elevations, if there were any, ought, according to the theory laid down above, to be peculiarly ſubject to the ſcurvy. But as no perſons paſs their lives in ſuch ſituations, we cannot expect from this quarter any deciſive facts one way or the other. Nevertheleſs, the only inſtance I know of a number of perſons continuing long in thoſe inhoſpitable regions, does actually ſupply a probability in favour of this doctrine. Mr. Condamine, during the operations of the French academicians, upon the ſummit of Pichincha, was attacked with ſcorbutic ſymptoms *.

(*) Un d'entre nous (Mr. D'Arcet ſays Mr. Condamine is meant) commença a reſſenter des affections ſcorbutiques, les Indiens et les autres domeſtiques que eurent des tranchees violentes; ils rendirent du ſang. (Bouguer, Mem. de l'Acad. R. des Sciences, 1744.)

The

The advantage to be derived from a better knowledge of the ſcurvy is, as in other caſes, to increaſe our power of preventing and curing it. And here a thouſand projects preſent themſelves.

1. The neceſſity of keeping ſhips ſupplied with freſh air becomes ſtrikingly obvious. And it would be worthy of another Lind to prove the truth of the theory, by ſupplying his patients with air containing a more than ordinary proportion of oxygene, which, however, to be done ſafely, muſt be cautiouſly attempted.

I was deſirous to know whether the temperature of ſcorbutic patients differed from the common ſtandard, but I find no obſervation of this ſort upon record.

2. We muſt build our hopes of prevention, and more eſpecially of cure, upon the acids. The efficacy of the native acids of vegetables is ſo ſtrongly atteſted by different writers down to the moſt recent, as Mr.

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Drinkwater *(Siege of Gibraltar)* and Dr. Trotter, that if an adequate ſupply could be provided and preſerved, no other means would be requiſite for the preſervation of ſoldiers and ſailors, whether only liable to the ſcurvy, or really attacked by it.

A full trial of the mineral acids, as they are called, eſpecially the nitric and vitriolic, ought, I think, to be made. The vitriolic elixir, the worſt poſſible form has never, even in the opinion of Pringle (l. c. 383.) been properly adminiſtered. Water, to which a ſmall quantity of the acid ſhould be added, would obviouſly be the beſt form. Had theſe obſervations been printed before thoſe of Dr. Trotter, I ſhould confidently have ſaid, that the vitriolic acid is decompounded in the ſtomach and bowels, and that the uſe of the nitric acid, as appears alſo from experience to be the caſe with the oxygenated marine acid, would require great caution, leſt in conſequence of its eaſy decompoſition it ſhould inflame the ſtomach. I read with no ſmall ſurprize in his

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his work (p. 147), that the nitric and ſulphuric acids, 'in whatever matter they are 'exhibited, paſs through the body pure 'and unaltered, as when taken, into the 'ſtomach.' He repeats the ſame aſſertion (p. 184.) with reſpect to the ſulphuric acid. It is however certain, that almoſt all animal and vegetable ſubſtances decompound theſe acids; and, in truth, Dr. Trotter has completely miſunderſtood the table in the *Methode de Nomenclature Chimique*, upon which alone he founds his opinion. In this table he ſays, 'are to be found thoſe 'bodies, of which oxygene is a *compound*,' (he means a conſtituent part) 'arranged ac-'cording to the degrees of elective attrac-'tion. At the top of the column is water, 'next follows nitric acid, carbonic acid, 'ſulphuric acid, &c. and not till after the 'tartarous acid come the oxalic, gallic, 'citric, and malic acids; hence theſe acids, 'by being more eaſily decompoſed, or their 'radicals and the oxygene being confined 'in weaker degrees of attraction, they are 'acted upon by the powers of aſſimilation

'and digeſtion of the human body; by 'which means they become ſubjected to 'the animal proceſs, and form new com-'binations with our fluids.' The latter part of this paſſage ſeems to conſiſt of mere words; at leaſt, the ideas are vague; and it is certain, that the French philoſophers have exhibited only a view of their new names in the column in queſtion, and not the comparative attractions of different baſes for oxygene, otherwiſe the acids muſt have ſtood in a very different order. My opinion of the decompoſition of vitriolic acid in the alimentary canal is founded upon the anſwer to queſtions aſked with a view to aſcertain this very fact. Perſons taking vitriolic acid I have found to be very ſenſible of the ſmell of ſulphur upon different occaſions.

Either therefore vitriolic acid ought to be efficacious, or ſome clear and ſatisfactory reaſon ought to be aſſigned for its want of power. The ſame obſervation I think applicable alſo to vinegar. Some obſervations

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in Lind ſeem to favour this expectation, as where he ſays, that 'the relief obtained 'by bathing the legs frequently in a day, 'with warm vinegar, is both *quick and ſur-* '*priſing*.' Did he make a comparative experiment with warm water?

If herpetic eruptions, * as there is ſome reaſon to ſuppoſe, depend upon an abſtraction of oxygene, leſs than that which occaſions the ſcurvy, or rather perhaps upon a local deficiency of this principle, one may preſume, that vitriolic acid, the beſt remedy we poſſeſs for the former, will not be inefficacious in the latter complaint. And although I am not led, by the improper application of a common name to confound two diſeaſes clearly diſtinguiſhed by their ſymptoms, yet it may, perhaps, without abſurdity, be propoſed as a query, whether

* The only circumſtance I cannot reconcile to this ſuppoſition is the efficacy of cantharides in theſe eruptions. The efficacy of certain metallic oxids, applied externally, favours it extremely.

they do not approach each other by a number of intermediate gradations. To illuſtrate my meaning, as well as to countenance my queſtion, I ſhall produce an example of a diſeaſe, which may, I think, be termed the *ſcorbutic leproſy, or herpes*; between which diſeaſes again there ſeems to me no certain boundary. Dr. Pallas obſerved this ſcorbutic leproſy in more than one place in the Ruſſian dominions. It makes a much ſlower progreſs than the ſcurvy. Thoſe whom it affects are not ſenſible, during the firſt and ſecond year, of any conſiderable pain or weakneſs. The countenance appears livid, as if they were ſtrangled; livid ſpots, little elevated, and giving no pain, are ſeen in different parts of the body; an herpetic eruption breaks out on the chin; the ſpots gradually enlarge, and violent pains are felt in the limbs; the herpetic eruption at laſt overſpreads almoſt the whole of the body, and to the bare, dark red ſpots ſucceeds a ſcurf, conſiſting of long ſcales, among which ulcers frequently

frequently break out. Sometimes the ſcurf falls off, and the ſkin takes on a natural appearance. If a perſon happens to wound or bruiſe any affected part, foul, ſpreading ulcers appear, and often eat down to the bone. The fingers are particularly liable to ſuch ulcers, and they commonly fall off joint by joint. This loathſome diſtemper, except in its very laſt ſtage, does not produce much emaciation or debility. The animal functions go on well. (Pallas, Reiſe, 302, 303.) The Tartars hold this, which they call, from the complexion of the patients, the *black* diſeaſe, to be infectious; yet the phyſician, who deſcribes it, ſaw a family, where the elder brother was firſt ſeized, the younger three years afterwards, the mother a year after him, while the wives of the two brothers, who had conſtantly lived with their huſbands, continued perfectly free. This obſervation will perhaps appear deciſive againſt the opinion of theſe poor people, eſpecially when we reflect how rapidly the logic of terror, among the ignorant in particular, haſtens to conclude, that

that ſuch and ſuch diſorders are catching. Dr. Pallas mentions a particular, in which this diſorder of the Coſſacks differs from the leproſy; the affinity however is ſtriking.

Theſe Tartars are utter ſtrangers to agriculture. They inhabit a country, where the ſoil is impregnated with ſalt, and abounding in ſalt lakes. Their diet is fiſh, often ſalted when it is more than half putrid. About the fort *Jaiſkoi Gorodoc* alone, watery fruits, as melons, and the *moſt indiſpenſable kinds of garden herbs*, are cultivated, but not by the Coſſacks, who muſt buy, if they will have them, and who, therefore, I ſuppoſe, ſupply themſelves but ſcantily.

3. SOOINS, an acidulous preparation of oatmeal, by which, as Pringle informs us (l. c. p. 382.), one of captain Cook's moſt intelligent friends cured his ſcorbutic ſick on board, deſerves more attention than it has obtained. It is prepared by pouring hot

hot water upon oatmeal, and ſuffering it to ſtand till it has become ſouriſh. The decanted water is to be boiled down to the conſiſtence of a jelly. The efficacy of this fermented acid affords a preſumption in favour of vinegar; and it may, I ſuppoſe, be generally procured on ſhip-board.

4. Nitre ſeems to deſerve attention in the cure of ſcurvy. It is doubtleſs decompounded in the *primæ viæ*, and capable of ſupplying much oxygene. Theſe fatal conſequences, which have ſometimes followed exceſſive doſes of nitre, have been, I imagine, owing to an inflammation of the ſtomach, occaſioned by the oxygene; and it is, probably, upon the direct or indirect action of oxygene, that the danger of expoſing wounds and certain parts of the body depends.

5. Sweet wort, or the *extract of malt*, I conceive, from its chemical compoſition, to have ſome, though inferior, antiſcorbutic virtues. And the great authority of captain Cook,

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Cook, who calls it one of the *beſt antiſcorbutics yet found out*, of Mr. Patten his ſurgeon, and others, ſtrengthens the opinion which chemiſtry ſuggeſts. It is true, as Dr. Trotter obſerves, that Macbride's theory is indefenſible; it is true alſo, that had there been nothing erroneous in his theory, wort, the unfermented and unfermenting extract of malt, could have had no efficacy in ſcurvy; neither has it been of late pretended that wort will of itſelf completely cure the ſcurvy; but from the conſiderable proportion of oxygene contained in ſugar, it is probable that it will retard the approach, and check the ravages of the diſeaſe. Dr. Trotter, I conceive, has much too lightly pronounced, that 'the ſimple wort paſſes 'eaſily through the body, and that it un-'dergoes no decompoſition in the ſtomach.' And, admitting this, does he ſuppoſe that none is abſorbed? that it tranſmitted no oxygene to the ſyſtem of thoſe hundred and thirty ſcorbutic patients, who took it for fourteen days, without a ſingle inſtance 'of ſickneſs, 'gripes, or purging,' occurring (Lind, p. 539.)

p. 539.) and who, though they were not cured by the wort, appear, as one may fairly conclude from Lind's expreſſions, to have grown better while they were taking it? It would, I think, be raſh and unjuſtifiable in the higheſt degree to lay aſide a preparation, at once cheap, eaſy, and palatable, and which appears to poſſeſs, in whatever light it is conſidered, powers of prevention by no means deſpicable. I am inclined to think much the ſame of ſour krout, notwithſtanding Dr. Trotter's objections.—The relation between a diet of ſalted proviſions and the ſcurvy, it is not eaſy to determine with preciſion. Such a diet is certainly not neceſſary to the production of ſcurvy; the facts already enumerated afford abundant evidence of the falſehood of this hypotheſis, and it has, of late, been generally abandoned. Nevertheleſs, ſalted meat may be capable of giving riſe to the diſeaſe, either by itſelf deſtroying looſe combination in which oxygene ſeems to be held by hydrogene and azote in the recent fibrous and cellular ſubſtance of organic beings, or by ſuffering

ſuffering a combination to be deſtroyed, in which perhaps oxygene is not a great deal more firmly held than by the azote of the atmoſphere.

The diet, therefore, of beſieged garriſons will, perhaps, account for the production of the ſcurvy, though the ſoldiers may not breathe the pureſt air in their barracks. Priſoners, eſpecially thoſe taken in war, are frequently attacked by this diſeaſe; and if we recollect how conſiderable their numbers have ſometimes been, and how cloſe their confinement, we ſhall be at no loſs to underſtand why they ſhould be viſited by this dreadful calamity.

One obſervation on the means of prevention and cure I will add before I quit the ſubject of ſcurvy. Of all the ſubſtances that can at once be cheaply procured and long preſerved, the concrete acid of tartar ſeems by far the moſt promiſing; it is very grateful, and comes near to the citric acid.

THIS

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THIS theory of ſcurvy ſeems, with ſome modifications, capable of being applied to the accumulation, or, more properly ſpeaking, the ſecretion of fat in exceſs. The oily parts of animals, I believe, principally differ from the reſt of the heterogeneous maſs, commonly called fleſh, in a ſingle circumſtance; they appear to contain a ſmaller proportion of oxygene. The actual convertibility of the ſofter ſolids into a ſubſtance like ſpermaceti, is a clear proof of the affinity between muſcle, cellular ſubſtance, and fat, as to their compoſition; add or take away a little of one or more of their conſtituent parts, and the alteration is effected. The converſion, as one may, I think, infer from the obſervations made in the burying ground of the Holy Innocents at Paris *, depends on ſome very ſlight cauſe.

The

* In the large *common graves*, where ſeveral hundred bodies lay together, all the ſoft parts were wonderfully

The diſtillation of organic matter, violent as that operation is, favours the ſuppoſition.

fully changed; in ſome ſubjects, where the proceſs of converſion was not finiſhed, a few of the muſcular fibres were viſible; in the reſt, all the muſcles, even the heart, the membranes, tendons, and viſcera had diſappeared; the brain was alſo changed. The ſubſtance, into which all was alike changed, was ſoft, ductile, greyiſh white, much reſembling new cheeſe. It frothed extremely with water, and proved to be an ammoniacal ſoap, from which acids ſeparated a concrete oil, which, when dried ſlowly, was like ſpermaceti, having a lamellated, cryſtalline texture; it was much more ſoluble in alcohol than ſpermaceti is, and it readily diſſolved in cold ammoniac. As it bore ſome reſemblance to wax, Mr. Fourcroy propoſes to call it *matiére adipo-cireuſe*.—The ſoap or *graſs* is ſaid, however, not to conſtitute above $\frac{1}{10}$ or $\frac{1}{12}$ of the body. The reſt evaporates in the form of water, carbonic acid air, and perhaps ſome unknown elaſtic fluids. (Annal. de Chimie, v. 154. viii. 17.)

Mr. Thouret appears to entertain ſome doubt as to the converſion. He ſays, they appear to be converted, but imagines the converſion may not be real. He ſeems to be perplexed by ſome indiſtinct hypotheſis lurking in his mind, and perhaps is not aware that converſion can only mean a change from the loſs or acceſſion of ſome conſtituent part. Except the bones, the colouring matter of the bile, of the bronchial glands, the pigmentum

fition. The water and carbonic acid, having appropriated the oxygene to themſelves, the oils, ammoniac, and carbone appear to be formed from the reſiduary azote and carbone. From Mr. Fourcroy's experiments, as well as from the proceſs of diſtillation, it looks as if the production of animal oil and ammoniac were operations nearly allied. One would not aſſuredly reaſon with much confidence from diſtillation to ſecretion, eſpecially as the diſtilled

mentum nigrum, 'and, perhaps, the *proper* ſubſtance of 'the muſcles, this transformation has entirely ſubdued 'all the other parts; the ſkin, adipoſe ſubſtance, mem-'branes, muſcles, viſcera, cartilages, glands, tendons, 'aponeuroſes, and even the fluids.' Again—'though 'the transformation ſeems to have taken place in the 'muſcles, it has probably eſtabliſhed itſelf in them by 'means of the lymphatic and unctuous juices.' (J. de Phyſique, Avril 1791, p. 256—7.) Mr. Hallé, in treating the fibrous parts of animals with nitric acid (ibid. Mai, p. 338—9.) obtained an oily concrete, ſimilar to ſpermaceti, and unalterable by nitric acid. If, as Mr. T. ſeems to argue, this were really the baſe of the animal fibre, it might eaſily happen, that during a certain ſtate of the ſyſtem it ſhould be changed into fat inſtead of muſcular fibre.

differ in appearance from the ſecreted oils, and doubtleſs alſo, ſomewhat in the proportion of their conſtituent parts. But is it unlikely that, if the blood itſelf ſhould ever contain but little oxygene, a ſubſtance containing a ſmaller proportion of oxygene than other animal ſubſtances, ſhould be formed? and if muſcle, membrane, tendon be changed into fat after death, why may not the powers of life, inſtead of muſcular atoms, occaſionally combine into fat thoſe elements, which, under other circumſtances, would have gone towards the formation of fibres or laminæ? In diſſection we avoid fat ſubjects, when we deſire to ſhew the muſcles diſtinctly. Is it that there is leſs muſcle as there is more fat? or are the fibres, the maſs of muſcle remaining the ſame, only rendered more indiſtinct? Every new hypotheſis, if it be but thought worth the trouble of refuting, adds to our ſtock of well-aſcertained facts. Till it be certainly decided, whether, as I ſuſpect, the fat encreaſes at the expence of the muſcle, let us try if we cannot diſcover, in the living ſyſtem itſelf, ſome obvious appearances,

pearances, indicating a tendency to form fat, whenever there is a deficiency of oxygene to a certain degree.

1. Among the Africans, of whoſe ſufferings on board the ſlave ſhip Dr. Trotter has given ſo particular and affecting an hiſtory, corpulence ſeems to have been, as it were, the firſt ſtage of ſcurvy. 'When a 'negro was becoming rapidly fat,' ſays he, 'it was no difficult matter to determine 'how ſoon he would be ſeized with the 'ſcurvy;' ſo that corpulence ſeems here to have been the harbinger of the ſcurvy. Writers have been particular in noticing that this diſeaſe ſeldom or never produces emaciation. Dr. Trotter, upon whoſe information we may place full reliance, tells us, that having purpoſely enquired among his medical acquaintance in the navy, he did not find one who conſidered the waſting of the fleſh or abſorption of fat as a ſymptom 'congenial to ſcurvy.' He immediately ſubjoins ſome obſervations of his own, that clearly indicate a connection between

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the ſcurvy and obeſity (p. 98, 99.). 'In 'a corpulent ſtate of the body,' he ſays, 'the moſt hideous features of the diſeaſe 'are expreſſed; ſuch are the bloated looks 'and countenance, &c.' In a meſs of midſhipmen, who lived altogether on the ſhip's fare, the only one he ever ſaw affected with the ſcurvy was 'a young man remark-'ably corpulent.' From the whole of his obſervations it appears clearly that obeſity pre-diſpoſed his patients to ſcurvy, or rather was to them what cachexy is to dropſy.

2. The emaciation produced by acids, which is exceſſive, where they are taken to exceſs, is a fact of which it is only neceſſary to remind the reader. In cyder countries the people are habitually leaner than where beer is the common liquor.

3. That inactivity which ſtands to obeſity in the relation both of cauſe and effect, generally prevents fat perſons from attempting to reduce themſelves; and though they may make a few ſtruggles at firſt, they finally ſubmit

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ſubmit to the incumbrance. They ſeldom conſider themſelves as in a ſtate of diſeaſe, and a prejudice prevails, that corpulence cannot be cured without danger. Hence we hear but little of the medical treatment of obeſity. The authentic facts, however, which we poſſeſs, ſhew that it may be ſucceſsfully treated in the ſame manner as the ſcurvy. This analogy is particularly ſtriking in two caſes of unwieldy corpulence, which Fothergill removed by a ſtrict vegetable diet. Mr. Wood, whoſe caſe is ſo well known from his own account, publiſhed in the Medical Tranſactions, is another inſtance of the ſame kind.

4. Short-winded perſons are very often corpulent, and even many aſthmatics. Wherever the livid colour * of the countenance

* I mean an occaſional or prevailing hue, not a permanently fixed ſtain, ſuch as the red noſe and cheeks of perſons poiſoned by fermented liquors exhibit. This deformity I ſuſpect to ariſe from inflamed lymphatics, which, by continued exceſs in drinking, become indurated. When a red noſe alternates, as it often does, with pain in the liver, are the abſorbents only or the

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nance indicates a deficiency of oxygene, there you will ſeldom fail to obſerve a full habit. Here I ſhall probably be told that I put the effect for the cauſe, ſince dyſpnœa is the conſequence of the accumulation of fat. Inſtances may alſo be produced of an emaciated habit of body, attended by difficulty of reſpiration.

In anſwer to the firſt objection one would not aſſert any thing poſitive; it is a ſubject on which it is not eaſy to make deciſive obſervations. It only appears to me, from my remarks on corpulent habits, that dyſpnœa and obeſity favour each other. I think it evident alſo that every different ſpecies of dyſpnœa, though in appearance

whole organized ſubſtance alike affected? My reaſon for ſuſpecting the lymphatics to be principally affected is not only becauſe the career of groſs debauchery ſo frequently terminates in dropſy, but becauſe I have been able to feel cords like ſchirrous abſorbents in various parts of the bodies of perſons deſtroyed by alcohol. Had ſo much induration been owing to ſchirroſity, lancinating pains muſt have been felt, which I have been aſſured by the patients was not the caſe; nor was there any cauſe whatever beſides theſe cords for ſuſpecting the preſence of ſchirrus or the approach of cancer.

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equally distressing to the patient, does not equally prevent the action of the air and the blood on each other.

It is not so difficult to elude the force of the other objection. The living body is doubtless subject to the influence of counteracting causes, of which that which tends to diminish the fat may prevail over its antagonist. Fat may be secreted without being accumulated. In a perfect theory, these counteracting causes, I am sensible, ought to be specified, and their power estimated; but we are so far from having attained such precision in the knowledge of any one of those various chemical operations that are comprehended under the improper term *secretion*, that it is making some advance to indicate a probable general cause, even if we are obliged to leave its action to be accurately determined hereafter. I object to the term *secretion*, taken in its proper sense, because many circumstances concur to render it probable, that only the elements of *secreted* substances exist in the blood, and that combination is the office of

the ſecretory organs. No one now, I ſuppoſe, will maintain that they are mere filtres.

5. The mechanical effect of exerciſe has never yet been fully appreciated. I have no thought of diſturbing the abſorbents in the poſſeſſion of their claims; their action is, I doubt not, increaſed by exerciſe. But may not exerciſe alſo, by introducing more oxygene into the ſyſtem *, and by diffuſing it more widely, check the formation of a ſubſtance containing little oxygene, while the fat, in common with the other fluids and ſolids, is abſorbed? In the courſe of life there are two periods during which the fat is apt to accumulate; the firſt is

* A very delicate obſervation made by Mayow is well deſerving of notice in eſtimating the effect of exerciſe. A dog that was panting and breathing deeply, on receiving arterial blood into one of his veins, inſtantly began to breathe ſo calmly that his reſpiration was ſcarce ſenſible. The animal received from an unuſual ſource the ſubſtance which is probably expended in muſcular action; it was therefore no longer neceſſary to inhale it rapidly.

that

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that of infancy, during which not inaction merely, but, if I am not deceived, the ſtate of the blood alſo, favours the formation in exceſs as well as the accumulation of fat. The lungs are not probably fully expanded for a conſiderable time after birth; and whatever quantity of blood goes to the aorta by the arterious duct, or to the left auricle by the foramen ovale, which frequently continues open for a conſiderable time, muſt ſo far prevent the oxygenation of the ſyſtem *.

The commencement of the ſecond is fixed by Haller at forty years of age. At this period of life an indolent diſpoſition is coming on; neither curioſity nor the other paſſions any longer agitate us with equal force. At this period I conjecture that there is alſo a deficiency of oxygene in the ſyſtem; and the conjecture will receive

* Cæcum *ductum arterioſum* reperi aliquando poſt paucos menſes, alias ſerius, ut ſecundo anno abſoluto tubulus pervius fuerit, *Haller.* III. 161. celerius adeo quam foramen ovale clauditur, ib. 162.

ſome

ſome confirmation from the papers of Dr. Girtanner; whether it be that the ſtomach and lungs are ſo altered as not to imbibe it in the uſual quantity, or the other conſtituent parts of the ſolids and fluids are ſo altered in their proportion as to have loſt ſomewhat of their attraction for oxygene, or upon whatever elſe it may depend. The leſſened vigour of the whole ſyſtem, the diminiſhed irritability of the muſcular fibre, the wearineſs that now ſo much ſooner follows exertion, indicate this, and the rigidity of the tendons, fibres, and laminæ may, in advanced age, be owing to a permanent, as, in ſcurvy, to a temporary deficiency of the ſame principle. If this ſuppoſition were juſt, might not ſome means be diſcovered to protract the period of youth and vigour indefinitely.—Whether true or falſe, and even though we ſhould never be able to reſtore new excitability to the ſyſtem, there can be no doubt of the immediate practicability of prolonging life conſiderably, and, what is much more deſirable, of maintaining a firmer ſtate of health, by a proper management

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management of the excitability during the periods of infancy and youth.

6. It is generally obſerved that perſons who indulge much in ſleep are apt to grow fat. From the well-known infrequency of reſpiration during ſleep, a ſmaller quantity of air muſt be taken into the lungs than while we are awake. The neceſſary conſequence and its application are obvious. The zoologiſts ſupply obſervations to the ſame purpoſe. Thus, *Mures montani hieme pinguiſſimi* fiunt, *etiam abſque cibo* * ; *idemque de urſis eſt notiſſimum* † ; *ut recte pingueſcant gallinæ, plurimum ut dormiant neceſſe*

* Haller Elem. Phyſiol. I. 39. For this fact he quotes Ray's *Wiſdom of God*, but, as I remember, erroneouſly. Ray, if I do not miſtake, ſays, that the marmot, the mus montanus here underſtood, is fat in autumn, and comes out lean in ſpring, juſt the contrary of what Haller makes him ſay. I do not therefore build upon what he aſſerts of the marmot or the bear: the other facts are to my purpoſe.

† Ariſtot. Hiſt. Anim. VI. 36. Hillerſtroem Iamtelands Diur. fang.

eſt,

eſt *, *eoque ſcopo ipſum lolium in cibum miſcetur* †.

Vegetables doubtleſs decompound water; it appears almoſt certain that they muſt combine the hydrogene with azote from the atmoſphere, to which a certain portion of oxygene is added. How then comes it to paſs that theſe elements are not at times ſo combined as to produce an exceſs of oil and a ſort of obeſity in the vegetables? Some ſuch modification of their functions does really take place. In proportion as they are more expoſed to heat and light, they ſeem to form a larger quantity of oils and reſins, as well as of ſaccharine matter, which is nearly allied in its compoſition to the two former ſubſtances. Wherever they are able to decompound moſt water, there alſo they will probably abſorb moſt azotic air. Muſt not theſe two functions go on moſt

* Reamur, Art de faire eclorre des oiſeaux domeſtiques, p. 2. id. p. 392.

† Id. p. 393.

vigorouſly

vigorouſly in thoſe plants, which in the ſame ſoil and climate form moſt oil? We know beſides, that vegetables are capable of forming oils, either exactly the ſame as thoſe of animals, or very nearly reſembling them. Thus we have the ſuet of the *croton ſebiferum*, the butter of the *phœnix dactylifera* and of the *butyrum cacao*. When, from a more intimate acquaintance with them, we ſhall be better able to apply the laws of organic bodies to the accommodation as well as the preſervation of life, may we not, by regulating the vegetable functions, teach our woods and hedges to ſupply us with butter and tallow? Thus our paſtures and meadows, the moſt fertile ſpots in every country, would, many of them, be gained to the cultivation of corn, the immediate food of MAN. And how many millions of inhabitants more might BRITAIN maintain, if we could feed upon the immediate produce of the ſoil? how many tons of vegetable food are condenſed into every fat ox?

WHEN

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WHEN the medical practitioner finds himself deserted, in every emergency, by the hypotheses of the schools, he is too apt to attach himself to a blind routine, and to conclude that attempts to explain the capricious phænomena of diseases, for so they must appear to him, will be hereafter as fruitless as they have heretofore been. The next step is to decry the value of that which he neither possesses nor hopes to attain; nor will he tolerate in others attempts to discover the laws of animal nature, of the existence of which it is yet impossible to doubt. Perseverance in the observation and comparison of phænomena, which have hitherto been observed and compared in vain, is the privilege of a few superior minds. Examples of ill success in these difficult investigations do not overpower their firm persuasion of the existence of invariable laws, and of the possibility of a compleat theory of diseases. I feel not much reverence for those who

pique

pique themſelves upon pure experience. There are few diſeaſes in which we have any fixed rule of practice; and our ſpecifics are ſo few, and ſo eaſily applied, that this part of medicine may be acquired without difficulty or loſs of time. In moſt inſtances, a theoretical deliberation of ſome ſort muſt precede preſcription, and here the diſcrimination of perſons habituated to ſpeculation will have the ſuperiority of ſkill over chance, and their fertility of reſources will appear to peculiar advantage.

In the conſumption of the lungs, as indeed in too many other diſeaſes, a conjecture may be offered with the leſs diffidence, ſince experience cannot here ſet up the ſlighteſt pretenſion to overawe ſpeculation. This melancholy truth will, I hope, propitiate thoſe whoſe diſpleaſure might otherwiſe ariſe againſt an attempt to diſturb medicine by the introduction of new ideas. I beg leave alſo to aſſure perſons little acquainted with the recent progreſs of ſcience, that I do not employ the French chemical nomenclature

clature from affectation; but as the French has prevailed over the old theory of chemiſtry, ſo I expect that the terms ſanctioned by the founders of that theory, however uncouth they may ſound at preſent, will finally eſtabliſh themſelves, partly as being more convenient, partly as being the language in which the moſt eminent philoſophers of Europe communicate their diſcoveries. Perſons acquainted with the ſubſtances they deſign will find no obſcurity in the terms, and for others it is indifferent what terms are uſed.

Not much ſeems to be gained by ranking the phthiſical tumours and ulcers of the lungs among ſcrophulous complaints. We have no very ſucceſsful method of treating ſcrophulous ſores, wherever ſituated; neither have we any tolerably clear idea of the nature of the diſeaſe. Not to mention that very different ailments are comprehended under a term ſo conveniently vague. I ſee no hopes therefore of transferring any uſeful ideas from the external appearance of

ſcrophula

ſcrophula to the internal form of the diſeaſe, if they ſhould be eſſentially the ſame, of which I am by no means ſatisfied. We ſee, it is true, ſtrumous perſons attacked by conſumption; but we may obſerve many others attacked by it, who have had no glandular ſwellings, or other marks of ſcrophula. However this may be, the only circumſtance in phthiſis, from which, in our preſent ſtate of ignorance, we can hope to reaſon to any purpoſe, has always appeared to me to be the *occaſional* effect at leaſt of pregnancy in ſuſpending the progreſs of phthiſis; for if we could once diſcover how pregnancy produces this ſingular effect, we might be led to diſcover alſo a method of ſuperinducing and prolonging the ſame change of the ſyſtem at pleaſure.

I had repeatedly attempted to proceed through the obſcurity by the help of this clue, but in vain. I have lately had a very favourable opportunity of obſerving this effect of pregnancy, but could fix on no plau-

ſible ſuppoſition, with which I might compare the phænomena. At laſt, when it was too late, the diſeaſe having returned and deſtroyed the patient, the following ſuppoſition occurred: The fœtus has its blood oxygenated by the blood of the mother through the placenta. During pregnancy there ſeems to be no proviſion for the reception of an unuſual quantity of oxygene. On the contrary, in conſequence of the impeded action of the diaphragm, leſs and leſs ſhould be continually taken in by the lungs. If therefore a ſomewhat diminiſhed proportion of oxygene be the effect of pregnancy, may not this be the way in which it arreſts the progreſs of phthiſis; and if ſo, is there not an exceſs of oxygene in the ſyſtem of conſumptive perſons? and may we not, by purſuing this idea, diſcover a cure for this fatal diſorder?

I am not able to prove the exiſtence of this *phlogiſticated* or *putreſcent* ſtate of the ſyſtem, as it would formerly have been called, during pregnancy. I muſt therefore appeal

appeal to the preſent knowledge or future obſervation of accoucheurs to determine whether it manifeſts itſelf in the colour of the blood, bleeding of the gums, dark coloured ſpots, vibices, or any other ſcorbutic ſymptoms. The ſtate of the blood in pregnancy is certainly peculiar; this the writers on midwifery declare in very pointed terms; but I have not found its ſenſible qualities ſo well defined as might be wiſhed. Thus we read that 'the blood of pregnant 'women is always found to have what is 'called a ſizy appearance, though of a pe-'culiar kind, and evidently very different 'from that which is obſerved in caſes of in-'flammation*.' I know not whether the ſpots or blotches mentioned by the ſame author †, or the red ſpots ‡ mentioned by Mauriceau as covering the legs and thighs, or the dark colour of the abdomen and inferior extremities deſcribed by Camper in his diſcourſe on the colour of the negroes, as often ſeen

* Denman, Introd. to Midwifery, p. 248.

† p. 266. ‡ p. 72.

in pregnant women, which Le Cat had alſo obſerved, are ſcorbutic blotches; and if ſo, whether they proceed from a conſtitution habitual during geſtation, and in theſe inſtances more ſtrongly marked than uſual. This conſtitution, if it exiſts at all, requires nicer attention than has probably been paid to it, both becauſe it is not likely in general to recede very far from the healthy ſtate of the ſyſtem, and becauſe ſome phænomena are ſeldom well obſerved, till they are obſerved with reference to an hypotheſis. The natural effect of an habitually ſtraitened reſpiration will have more weight with the ſkilful and reflecting reader than this lame and defective evidence. The following coincidence he will perhaps think remarkable. Pregnant women agree with ſcorbutic patients in that ſtrong inſtinctive appetite for vegetables, and it appears as if this diet was the moſt ſuitable to them. 'Pregnant women,' ſays Dr. Denman, 'have generally 'a diſlike to animal food of every kind, and 'under every form—on the contrary, they 'prefer vegetables, fruit and every thing

'cooling, which they eat and drink with 'avidity, and in which they indulge without prejudice *.'

But is the inference from the ſuppoſed deficiency of oxygene in pregnant women confirmed by appearances about phthiſical patients? Do they ſhew any ſigns of a redundance of this principle?

The clear, bright, and florid hue of the fluſhed hectic countenance, ſo diametrically oppoſite to the ſcorbutic complexion, affords ſome preſumption of a ſtate of the blood, equally receding, but in an oppoſite direction, from the ſtandard of health. The countenance of perſons fluſhed by exerciſe or food widely differs from that vermilion bloom, which is miſtaken by the uninſtructed for the ſign of health, though it is the harbinger or attendant of an incurable diſeaſe †. On holding the hand of a conſumptive

* Introduct. to Midwifery, p. 249.

† The clearneſs of ſkin in perſons ill of phthiſis, or

ſumptive perſon againſt the light, the ſemi-tranſparency of the margin of the fingers and joints is, I think, evidently of a more vivid carnation in conſumptive patients. A ſtriking difference would probably be perceptible between the hand of a phthiſical and ſcorbutic patient. In the former, the appearance may, indeed, be imputed to emaciation; but there are ſimilar appearances which cannot by any means be imputed to emaciation, nor, in my opinion, to any cauſe except that aſſigned above. 'During the fever,' ſays a late attentive writer, who did not here look through the coloured medium of hypotheſis, 'the cheeks appear as if 'painted with a circumſcribed ſpot of *pure* '*florid red*; the lips and the tubercles in

pre-diſpoſed to it, is a general obſervation. *Qu.* When it attacks perſons who have herpetic eruptions, do thoſe eruptions ceaſe?—Is not the chillineſs of ſcorbutic patients, and of the *blue boy*, owing to a want of oxygene? this chillineſs is perfectly diſtinct from febrile horror and rigor.—And is not that *ſenſe* of heat, which ſo much diſtreſſes phthiſical patients, owing to *actual* heat, produced by the exceſs of oxygene?

'the canthus of the eyes are alſo redder 'than when in health *.' Again, towards the termination of the diſeaſe, 'the tongue 'appears clean; and, with the fauces, is of a '*bright red*†.' Here I requeſt every reader acquainted with the modern doctrine of reſpiration (otherwiſe he is not a proper judge) to pauſe a moment. It will, I imagine, readily be allowed that if the blood-veſſels were filled with a more florid blood than uſual, this identical bright redneſs would be ſeen. The cauſe, then, is adequate to the effect: the change, too, is not, as in the looſe analogies of the humoral pathology, tranſported from ſome remote part of inanimate nature, and at random imputed to animals. If the various evidence adduced in the caſe of ſcurvy ſhould not prove ſatisfactory, yet the moſt determined ſceptic will not heſitate to admit in aſthma a deficiency of oxygene and a correſponding change of colour. Why then will he not infer from an oppoſite change of colour an

* Reid's Eſſay on Phthiſis Pulm. ed. 2nd, p. 13.

† Ibid. p. 19.

oppoſite condition of the blood? If in the *puer cæruleatus* of Dr. Sandifort, the tongue and fauces were deep blue or purple, becauſe the lungs did not admit oxygene enough, will it not be granted that a bright red indicates the reception of too much?

'But then the blood itſelf ought, accord-'ing to your ſuppoſition, to be brighter than 'ordinary;' the arterial blood certainly; and a compariſon of the arterial blood of phthiſical and healthy perſons would be ſo intereſting on this very account, that I wiſh it were practicable. But if we conſider the whole action of oxygene upon the blood, we ſhall not perhaps expect the difference to be ſenſible on a compariſon of the venous blood; for oxygene ſometimes darkens and ſometimes enlivens the venous blood; not, however, indiſcriminately, but under determinate circumſtances. When blood is expoſed to oxygene air, it firſt becomes florid, and afterwards black. Mr. Fourcroy and Mr. Haſſenfratz * have aſcertained this

* Annales de Chimie, t. IX,

fact

fact in a very careful manner; and I have had ſeveral opportunities of remarking the ſame change. The oxygene, which at firſt is combined with the whole maſs of the blood, or the red globules at leaſt, after ſome time is attracted by the hydrogene alone or united with azote, and forms accordingly water or carbonic acid. If oxygene be added in a large quantity at once, or looſely combined, the blood is never brightened, but turns immediately black, as when oxygenated marine acid is added to blood.

Probably the ſolids, during circulation, more than divide with the blood its looſely attached oxygene; if they have a ſuperior attraction, they will, as ſome of the conſtituent parts of the blood itſelf do upon ſtanding, take the whole and leave the blood dark-coloured.

Hence then it appears poſſible enough that an unuſual quantity of oxygene being thrown in through the lungs, the ſolids might attract it and be conſumed themſelves,

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ſelves, or prepared for abſorption in ſome way not underſtood, while the venous blood returns to the lungs of its ordinary colour; and, were its colour changed, the difference in the ſhade might not be obvious; beſides, ſome of our chemical diſcoveries are ſo recent, that time has been wanting to apply them to improve medicine extenſively; ſo that ſatisfactory comparative remarks on the colour of the venous blood in conſumptive patients are not to be expected ready made. We ſeldom perceive much more obvious phænomena than this probably can be, till we are prompted to look for them.

From Willis to Fothergill, and from Fothergill downwards, ſcarce any real obſervation occurs upon the ſtate of the blood. One tells us that it is polluted, another that it is contaminated, a third that it is acrid, a fourth that it is putreſcent *, without ever recollecting

* Leſt it ſhould be ſuſpected that *putreſcent* is here a ſound ſignificant, it ſhould be obſerved that 'when 'we examine the blood drawn from patients in every 'period of conſumption, ſo far from any appearance 'of

recollecting that to employ terms expreſſive of phænomena, ſuch as the ſenſes may recognize and to reaſon upon ſuch phænomena alone, are indiſpenſable conditions in philoſophizing.

Finding ſuch little ſatisfaction in the writers whom it was in my power to conſult, I applied to a gentleman whoſe opportuni-

' of diſſolution in its contents, the reverſe is conſtantly ' found; a *thick buffy* ſize and *firm* craſſamentum. ' Nay, ſo inconſiſtent are the favourers of this doctrine' (i. e. of the putrefaction of the fluids and ſolids in certain diſeaſes) ' that the *ſize*, and the degree of coheſion ' in the blood, has always been eſteemed an indica- ' tion that the operation ought to be repeated, and ' much blood has been unneceſſarily ſhed accordingly. ' Nor in its progreſs do we obſerve any ſymptoms ' ſimilar to thoſe found in diſeaſes uſually termed pu- ' trid; *no petechiæ, vibices, ſordes about the teeth, or blood ' iſſuing from the gums, and other parts of the body.*' Reid, p. 71—2. Dr. —— of Edinburgh, whoſe experience in blood-letting is immenſe, uſed to teach that in phthiſis there is an unconquerable morbid tendency in the ſyſtem to generate blood.—The firm craſſamentum, according to Dr. Girtanner, is a ſure indication of exceſs of oxygene.—No craſſamentum from ſcorbutic blood—none from the *blue boy's* blood: a ſtriking coincidence!—oppoſite ſtate in phthiſis not leſs ſtriking.

ties

ties of inſpecting the blood of phthiſical patients have been ample, and whoſe fine experiments on frigorific mixtures atteſt his talents for obſervation, and will diſpoſe the public to confide in his accuracy. I aſked him ſimply, without any previous communication, whether he had noticed any particular appearance in the blood of phthiſical patients. 'Yes,' he replied, 'and that 'ſo conſtantly, that I believe I could ge-'nerally diſtinguiſh blood taken from ſuch 'patients, eſpecially where the diſorder is 'confirmed.'—On the following written anſwer, he obſerved that colours are not eaſily deſcribed, and, perhaps, his terms might not be the beſt choſen: *florid* and *purple* ſeem indeed not well to agree.

DEAR SIR,

IN anſwer to your queſtion whether I recollect to have obſerved any particular appearance in the blood from phthiſical patients, I can inform you that I have always been as it were involuntarily, and without any particular deſign of attending to

to it, ſtruck with its thin conſiſtence, and its florid purpliſh colour.—Upon ſince putting the ſame queſtion to a foreign gentleman, who has paid particular attention to its appearance, and even made experiments upon it, he replied, that it differed from other blood in being thinner, more florid, and having a purpliſh hue.

&c. &c.

Oxford,
July 25th, 1792. RICHARD WALKER.

This foreign gentleman happened to viſit the Radcliffe Infirmary ſoon after my converſation with Mr. Walker. He has deduced ſome peculiar opinions on the nature of phthiſis from his reſearches on the blood taken from perſons labouring under the diſeaſe: theſe opinions, I underſtand, are quite different from that which I have propoſed, and indeed utterly foreign to it.

It is well known that the ſymptoms of phthiſis have been greatly aggravated in ſome patients who have been made to reſpire

ſpire oxygene air. Mr. Fourcroy deſcribes the reſult of the trial of oxygene air upon twenty patients, of whom he ſaw eleven himſelf. After a few flattering appearances, which inſpired them with very ſanguine hopes, they were all ſenſibly the worſe for this treatment, and as ſenſibly relieved by abandoning it. 'Even amid their ſelf-congratulations,' ſays he, 'ſeveral ſigns admo-'niſhed the attentive phyſician that their 'hopes were ill founded. The ſkin was 'dry and hot; the face took fire and be-'came of a more florid red, *s'allumoit et ſe 'coloroit d'un rouge plus vif qu'il n'etoit au-'paravant.*' This heightening of the colour by the inſpiration of oxygene air depoſes ſtrongly in favour of the opinion I am maintaining. Since the complexion, already more florid than natural, is heightened by the addition of oxygene, may we not conclude that the firſt gradation is alſo owing to an exceſs of oxygene. 'The ſymp-'toms' Mr. Fourcroy goes on to inform us, in a fortnight or three weeks after the firſt ſeemingly favourable effect of the oxygene air

air 'became all at once more ſevere; the 'change was indicated by a dry convulſive 'cough, ſpitting of blood, a ſenſation of 'burning heat and ſharp pain in the thorax, a 'fever almoſt acute and threatening to be-'come inflammatory, by agitations in all 'the members, reſtleſſneſs, and thirſt. 'It was neceſſary to bleed, to give anti-'phlogiſtic and ſedative remedies, and the 'patients ſhewed great unwillingneſs to in-'ſpire the oxygene air. When theſe violent 'and alarming ſymptoms were allayed by 'proper treatment, the diſeaſe reſumed its 'ordinary form, and the fever appeared with 'its quotidian type; the expectoration be-'came purulent again. In its 4th ſtage the 'diſeaſe made a quicker progreſs than uſual. 'This accelerated progreſs, the ſymptoms of 'inflammation, the uneaſineſs, the oppreſſion, 'the burning (*ardeur*) of the lungs, the ſtop-'page of the expectoration, the acute hæ-'moptyſis, all theſe phænomena were ma-'nifeſtly owing to the uſe of oxygene air. 'They equally took place in eight patients 'who were not ſo far gone as the others;

'and

'and it was neceſſary to abandon this mode 'of treatment—the patients themſelves in-'deed deſired that it might be abandoned*.'

In the appendix to one of Dr. Prieſtly's volumes on air, ſome caſes are mentioned of phthiſis, in which the patients were ſenſibly relieved by breathing common air largely mixed with carbonic acid air. Dr. Percival tells us that he has 'adminiſtered 'fixed air in a conſiderable number of caſes 'of the phthiſis pulmonalis, by directing 'his patients to inſpire the ſteams of an 'effervеſcing mixture of chalk and vine-'gar, or vinegar and potaſh. The hectic 'fever has been conſiderably abated, and 'the matter expectorated has become leſs 'offenſive and better digeſted. I have not,' he adds, 'yet been ſo fortunate as to effect 'a cure. One phthiſical patient has, by 'a ſimilar courſe under Dr. Withering's 'care, entirely recovered, another was ren-'dered much better, and a third, whoſe

* Annales de Chimie, iv. 85.

'caſe

'caſe ſeemed to be truly deplorable, ſeemed 'to be kept alive by it more than two 'months *.'

We cannot be ſurpriſed that theſe experiments ſhould not have been attended with greater ſucceſs, if we conſider that thoſe who made them could not at that early period be enlightened by the grateful dawn of a probable theory; that having no well-defined end in view, they could not vary their means with ſufficient intelligence; and that, where the apparatus was ſo awkward, ſufficient perſeverance could not well be expected. If our object be to lower the ſtandard of the atmoſphere, carbonic acid air will not probably be choſen for this purpoſe. Should it be objected, that the abſtraction of the oxygene was not continued long enough for the effect to be produced in this way, it may be replied, that in Mr. Fourcroy's experiments the application of oxygene was not probably continued much longer.

* Prieſtly, I. 301.

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Some ſmall probability ariſes in favour of this theory, from the inconſiderable number of ſailors who die of phthiſis, unleſs the common books, as thoſe of Lind, Rouppe, Blane, &c. from which alone I draw my information, have impreſſed me with wrong notions on this point. Seafaring people are particularly expoſed to wet and cold, the exciting cauſes of phthiſis; a large proportion of them are, I ſuppoſe, of ſuch an age as, according to the common eſtimate, is moſt liable to be attacked by conſumption, and yet they ſeem to be even peculiarly exempt from this diſeaſe. In the ten years' regiſter kept by Mr. Gorſuch, at Shrewſbury, upwards of one fourth of the deaths appear under the title, conſumption. Though the bills of mortality are inaccurate in their denominations, yet their authority is ſufficient to prove, that vaſt havoc is made by conſumptions in London. From the accounts and liſts of the ſick and dead on ſhip-board, conſidered with the neceſſary attention to the great difference of circumſtances, I ſhould not conceive ſo formidable an idea

of

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of the ravages committed among ſeafaring perſons by this diſorder; before any ſafe judgment can be formed, however, it would be neceſſary to have more preciſe data than I have met with, and eſpecially to aſcertain how far it is common for perſons ſeized with conſumptions at ſea to die on ſhore.

Scorbutic perſons ought not, according to this theory, to be liable to phthiſis, nor phthiſical to ſcurvy; at leaſt it ſhould ſeem, that as one of theſe diſeaſes comes on, the other ſhould retire. It does not appear to me, that we have well-aſcertained facts enough here to afford a teſt of the truth or falſehood of the foregoing reaſoning. Dr. Lind, indeed, ſays, that 'perſons very 'much emaciated with the *flux* or *conſump-* '*tion* are ſeldom or never ſeized with the 'ſcurvy *.' I ſuppoſe ſo careful an obſerver would uſe terms accurately, and more confidence may, perhaps, be placed in his experience in the ſcurvy, than in that of all

* p. 508.

other writers put together. But in Mr. Ives's Journal it is ſaid, that 'five or ſix ſcor-'butic men, who had coughs, are now in 'deep conſumptions *.' Again he ſays, 'ulcerated lungs is a common conſequence 'of the ſcurvy †.' Dyſpnœa, tightneſs and pain of the breaſt with coughing, are among the ordinary ſymptoms of the ſcurvy; nor will this appear extraordinary to a perſon who conſiders that neither the lungs, nor the left auricle and ventricle of the heart, perform their functions properly. It is therefore very poſſible to confound the pulmonic ſymptoms of phthiſis with thoſe of ſcurvy, though they ſeem perfectly diſtinguiſhable, by the abſence of hectic fever in the latter caſe, and by their yielding to vegetables. I have already endeavoured to ſhew, that very frequently the immediate cauſe of death in the ſcurvy is the diſeaſed ſtate of the contents of the thorax; and if all circumſtances be impartially conſidered, we ſhall conclude that Mr.

* p. 92.

† p. 107. May there not be *ſcorbutic* ulcers of the lungs?

Ives's

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Ives's account is not explicit enough to satisfy us, what effect phthisis produces upon the scurvy; whether the two disorders go on together, and whether the ulcerations of the lungs are such as take place in phthisis, accompanied with tubercles, &c. It would be curious to know how the slow and feeble pulse, natural to scurvy *, is modified by those causes which in phthisis render the pulse so frequent and so hard. Lind relates, from his own experience, that in the *Salisbury* man of war, in May 1747, 'when there prevailed several inflammatory 'disorders, particularly peripneumonic fe-'vers, or inflammations of the lungs, all who 'were recovering from them became highly 'scorbutic †.' This observation is so far from forming a difficulty in the way of any part of the preceding theory, that it remarkably confirms what has been said of scurvy. Those who die of pneumonia die, I believe, of suffocation; the countenance appears bloated, discoloured, and li-

* Lind, p. 108. † p. 74.

vid: even when the expectoration, upon which ſo much depends, goes on favourably, we may reaſonably ſuppoſe that leſs oxygene will be received through the lungs than in perfect health, during that debility of the thoracic organs which ſucceeds the ſtate of inflammation. Hence the ſcurvy will eaſily come on, when its exciting cauſes are at hand.

I do not know whether the emaciation preceding and accompanying phthiſis will be allowed to afford me any additional preſumption. Conſumptions have been ſaid to originate from the abuſe of vinegar and ſour fruits; but here I believe there is an inaccuracy in terms. In ſome inſtances of the diſorder ariſing from this cauſe, I have obſerved it to affect the abdomen, and not the thorax. The ſtomach and bowels are probably burned by the long continued application of even weak acids; and hence atrophy enſues, as the chylopoietic viſcera become unfit for the office of nutrition.

The

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The beneficial effect of the mineral acids in allaying the hectic ſymptoms for a ſhort time would be a contrary probability, if acids have not ſome immediate action, independent of their compoſition. The uſe of nitre in incipient phthiſis, if nitre, which I doubt, be really beneficial, would alſo form another objection.—It would therefore be raſh to place much confidence on ſo incomplete a theory, however ſtrongly it may ſeem to be favoured by ſome of the principal phænomena. If I might even take it for granted that exceſs of oxgene is a well-aſcertained circumſtance in phthiſis, it would ſtill remain to be determined, before the inveſtigation could lead to any thing uſeful, what rank it holds among the other deviations from a ſtate of health obſervable in this diſeaſe.

Here two ſuppoſitions occur: 1. The phthiſical inflammation may ſo alter the ſtructure of the lungs, as to cauſe them to tranſmit a more than ordinary portion of oxygene to the blood; or, 2. Some un-

known cauſe having enabled them to tranſmit, or the blood itſelf to attract, more oxygene, an inflammation of the lungs might enſue *.

The following obſervations of Mr. Lavoiſier may perhaps aſſiſt the reader's reflections, as well as illuſtrate ſeveral points of the preceding diſquiſition. That great philoſopher had confined a guinea pig for an hour and quarter in 248 cubic inches of

* During ſthenic inflammation, does not a too rapid combination of oxygene take place, of a kind ſimilar to the ſecondary combination deſcribed above, in conſequence of which blood changes from florid to black? May not the heat attending inflammation depend upon this ſecondary combination? Does not the livid colour ſucceeding violent inflammations countenance this ſuppoſition? or is that colour ſolely owing to ſtagnant extravaſated blood, in which the oxygene undergoes this ſecondary combination? We muſt ſeek the explanation of the mechaniſm of ſuch changes in the principles of chemiſtry; and the knowledge of this mechaniſm will not fail to be uſeful. If, for inſtance, the above hypotheſis were true, it would not be difficult to draw from it ſome practical inferences reſpecting the prevention of gangrene.

oxygene

oxygene air. 'Towards the close of the 'experiment,' says he, 'the animal ap- 'peared to suffer considerably. Neverthe- 'less, only a very small portion of the air 'was vitiated, i. e. converted into fixed air; 'there remained, after the animal was re- 'moved, much more vital air than is ne- 'cessary to constitute a salubrious atmo- 'sphere; this circumstance,' viz. the distress and death of animals confined in oxygene air, long before it had become unfit for respiration, 'had been noticed by Dr. 'Priestly. Having occasion to repeat some 'of his experiments, I chose Guinea pigs 'for the purpose: the air in which they 'were confined, was nearly pure; it did not 'contain above five or six parts in an hun- 'dred of azotic air. Although the animals 'lived much longer in this than they would 'have done in an equal volume of atmos- 'pherical air, they nevertheless all died 'long before it was compleatly vitiated; 'and other animals introduced into the air 'in which they had died did not appear to 'suffer, at least for some time. It is not

'therefore for want of air fit for reſpira-
'tion that animals die in vital air, but from
'ſome noxious effect of that air.

'Dr. Bucquet aſſiſted at ſome of my ex-
'periments; and we opened the animals
'that had died in vital air. In every in-
'ſtance death ſeemed to have been occa-
'ſioned by an ardent fever, and an inflam-
'matory diſeaſe. The fleſh was of a *very*
'*red* colour; the heart livid, and turgid with
'blood, eſpecially the right auricle and ven-
'tricle; the lungs were very flaccid, but
'*very red*, even externally; they were alſo
'turgid with blood.' (*Mem. de la Societé R. de Medicine*, t. V. p. 575—576.)

The unuſual animal heat, which muſt have been generated in theſe experiments, the ſtimulant power, which, independently of the heat, oxygene confers upon the blood, that irritability which it communicates to the ſolids; all theſe cauſes might eaſily produce the inflammation obſerved by Mr. Lavoiſier. Now may not the flower and

differently

differently modified inflammation of the lungs, in phthiſis, originate from a ſmaller exceſs of oxygene thrown into the ſyſtem in a more gradual manner?

According to the former of the two ſuppoſitions ſtated above, we might hope ſometimes to ſucceed in curing the diſeaſe by withholding oxygene, and giving the pulmonary ulcers an opportunity to heal; according to the ſecond, the diſeaſe would be ſtill more in our power; by removing the cauſe that produces and continues it, we might, with greater certainty, expect the inflammation to ſubſide.

Of theſe hypotheſes, I think it ſome recommendation that they lead to a project totally different from the nugatory modes of practice heretofore employed. The treatment they ſuggeſt is ſo obvious, that it is ſcarce neceſſary to add a ſyllable on the ſubject. Fruits, herbs, milk, &c. with all their cooling and all their occult qualities beſides, have never, I ſuppoſe, effected a cure of phthiſis; nor am I acquainted with

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with any reaſon capable of ſatisfying a perſon at all ſolicitous in forming his opinions to diſcriminate truth from falſehood, that they have ever contributed towards a cure. While the diſeaſe is forming, indeed, at which time the diſorder ſeems to be highly inflammatory, an oppoſite diet may accelerate its progreſs. But there will, probably, be little difficulty in prevailing upon men of reflection to avoid both a vegetable and a ſtimulating diet; and to put their phthiſical patients upon ſuch a diet as, according to the idea of that diſeaſe already ſo frequently repeated, ſhall tend to produce the ſcurvy. Not only ſalted meat, but an oily diet, may be tried. It will not however, I imagine, avail us much ſolely to cut off the ſupply of oxygene by the ſtomach. The lungs themſelves being diſeaſed, and alſo being the moſt copious ſource of oxygene, it would be moſt advantageous to ſupply them with an air ſuited to our purpoſe; ſuch an air ſhould be mixed either with an additional quantity of azotic or with hydrogene air, which ſeems to have no irritating quality, and has been found to have the

power

power of darkening the colour of the blood. We cannot expect benefit from the air of a crowded room, since its temperature may counteract the effect of its diminished proportion of oxygene. It is possible, but by no means certain, that the steams abounding in such a room, which have been complimented with the title of *putrid*, may be injurious to consumptive persons. Till some means of lowering the standard of atmospheric air, without adding to it any thing hurtful, shall be contrived, we may remove phthisical patients out of those airy spacious apartments which of late have been thought salutary in all diseases indiscriminately. They may at least sleep in confined rooms; and the more confined the better, provided a cool temperature be maintained.

Here it may be asked, with reference to this practice, whether consumptions destroy a larger proportion of the inhabitants of the town or the country. I shall have occasion to say a few words on this subject below.

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below. In the mean time, we can ſcarce expect any effect from the ſtate of the atmoſphere, ſince it appears to contain an equal quantity of oxygene air in the moſt populous city and the cloſeſt weather *.

It has ſometimes been ſuppoſed that hœmoptoe and phthiſis have been produced by quickſilver †; and this fluid metal has been

* According to the experiments of Mr. Scheele and Cavendiſh.

† In the ſcurvy, preparations of quickſilver in extreme ſmall quantity produce a copious and dangerous ſalivation, almoſt always attended with bloody ſtools. (*Lind, p.* 111.) A tendency to ſalivation is frequently obſerved in ſcorbutic perſons, independent of quickſilver. The chemical condition of the ſyſtem, I ſuppoſe, is the cauſe of the firſt-mentioned appearance. But in what this conſiſts, I cannot form any ſatisfactory conjecture at preſent. I think it alſo not preſumptuous to expect from chemiſtry the explanation of the peculiarity, which appears in ſome perſons, whoſe ſkin is no ſooner touched with quickſilver ointment than it is felt in the ſalivary glands. Moſt idioſyncraſies depend probably upon ſomething peculiar in the chemical compoſition of the ſyſtem. Of ſuch effects there muſt be determinate cauſes; and I ſee none ſo likely

been imagined to increaſe the momentum of the blood ſo much as to break through the looſe and tender veſſels of the lungs. This is a very clumſy account of the beginning of phthiſis, and ſuch groſs mechanical ideas are doubtleſs inapplicable to the motions of the living ſyſtem. Quickſilver is taken in oxygenated, and thrown out reduced; and the properties of oxygene, and its aſcertained connection with the functions of animals, ſeem to afford a far more appropriate explanation of the phænomenon.

The fact, if it might be explained in this way, would corroborate a ſuppoſition which is ſuggeſted by a common appearance; the ſuppoſition is, that the hyper-oxygenated ſtate of the ſyſtem precedes thoſe ſymptoms which characterize phthiſis. I am inclined to believe that the fatal and peculiar inflammation of the lungs, whether indicated by acute pains in the cheſt, ſpitting of pus, &c.

likely as this: I do not, therefore, wonder that theſe phænomena have hitherto appeared ſo unaccountable; for we have totally wanted data to explain them.

is poſterior to the accumulation of oxygene. In perſons only threatened with conſumption, and before any formidable ſigns of diſeaſe in the thorax occur, we often obſerve hæmorrhages from the noſe (in which I have thought the blood preternaturally florid) as well as a remarkable brightneſs of the complexion. Thus Mr. Portal, the celebrated anatomiſt, and one of the lateſt obſervers, ſpeaking of perſons, *menacés de tomber dans la phthiſie*, mentions the *rougeur ſouvent habituelle de leur viſage* *: this indeed is a very common place obſervation.

I add a general conſideration, which may poſſibly induce the phyſiologiſt to weigh the pretenſions of the foregoing hypotheſis with deliberation. The functions of the lungs ſeem to have been determined by modern experiments with great preciſion; they are deſtined to tranſmit oxygene to the blood, while at the ſame time a quantity of carbone and oxygene paſſes off in a contrary direction, and uniting with the oxy-

* Eſprit des Journeaux, Mars, 1792, p. 362—3.

gene

gene of the atmoſphere, forms carbonic acid and water. Hence one might conjecture that the lungs will be apt to deviate from an healthy ſtate in two oppoſite ways; in one of their morbid conditions, the combinations that take place within the thorax will be impeded; in another, they *may* be carried on to too great an extent; and then, the lungs being the principal focus of animal heat, they might be injured by being conſtantly expoſed to too high a temperature, or by having too much oxygene offered to the attractive power of their own ſubſtance.

Here I foreſee, without dreading, a ſpecious objection. 'Since the lungs are diminiſhed during the progreſs of phthiſis, is it 'likely that they ſhould, with a narrower 'area, carry on, to too great an extent, the 'combinations to which they are deſtined? 'Is it not alſo to be expected that the inflam-'mation ſhould render them leſs permeable?' It might be replied, that the great miſchief ſeems to be done before they are either

much thickened by inflammation, or wasted by absorption. Besides, it is common enough for opposite conditions to exist either throughout the whole system, or in a particular organ, at different periods of a disease, as excess of action is followed by debility. I however think it probable that instances have occurred, where the loss of a considerable part of the substance of the lungs has checked the progress of the disease. I knew a person who died, after a tedious struggle, of phthisis. The ribs on one side were pressed quite inwards, in consequence, I suppose, of the destruction of the corresponding lobe of the lungs. From this time, the disease, which had at first proceeded at its ordinary rate, went on wonderfully slow; and it was not till several years after the depression of the ribs that the patient died, without ever having had a distinct and long continued intermission. However this may be, in the generality of cases actual observation shews the objection to be groundless. In the various shades of livid colour, which are produced by a total

or partial excluſion of that portion of oxygene, which enters the blood by way of the lungs, we have nearly as good a criterion of any conſiderable deficiency of this principle as we can deſire. Now I believe it ſeldom or never happens that a livid ſuffuſion overſpreads the countenance of a phthiſical patient: *les rougeurs*, ſays Mr. Portal, *augmentent preſque juſqu' au dernier moment*; and in proportion as the dark-red aſthmatic fluſh is a ſtranger to this diſeaſe, we may infer that even the phthiſical dyſpnœa does not prevent a full ſupply of oxygene.

The more you reflect, the more you will be convinced that nothing would ſo much contribute to reſcue the art of medicine from its preſent helpleſs condition, as the diſcovery of the means of regulating the conſtitution of the atmoſphere. It would be no leſs deſirable to have a convenient method of reducing the oxygene to 18 or 20 in 100, than of increaſing it in any proportion. The influence of the air we breathe is as wide as the diffuſion of the

blood. The minuteſt portions of the organs of motion, ſenſe, and thought muſt be affected by any conſiderable change in this fluid. Whether it be that the brain muſt be waſhed by ſtreams of arterial blood, or that the action of every organ is a ſtimulus to the ſyſtem in general, and conſequently to every other organ in particular, it is certain, that when the acceſs of oxygene is cut off from the lungs, the functions of the brain ceaſe: perhaps there may be a mixture of azotic and oxygene airs more favourable to the intellectual faculties that that which is found in the atmoſphere; and hence chemiſtry be enabled to exalt the powers of future poets and philoſophers. That diſeaſes of excitement on the one hand, and debility on the other might be cured almoſt ſolely by a proper air one can hardly doubt, as well as ſeveral diſorders at preſent highly dangerous or deſperate, which one cannot, upon the faith of any obvious phænomena, refer to either head. The materia medica might, therefore, undergo a ſtill greater reduction, than it has lately undergone in conſequence

of

of the purification of medicine from its groſſer abſurdities; and hence the treatment of diſeaſes be at once rendered infinitely more pleaſant and more efficacious.

The extreme frequency of conſumptions in Great Britain ſeems to have been juſtly aſcribed to the variableneſs of the climate. A few reflections will, perhaps, render it probable that the cauſe is adequate to the effect, at leaſt that it is peculiarly calculated to excite pulmonary inflammations, with the concurrence of other circumſtances. Climates of equable warmth appear to be moſt favourable to health, and much the moſt congenial to the human race. Next to theſe may be placed thoſe climates which have fixed ſeaſons; the moſt deſtructive are the variable. In England we ſeldom enjoy any continuance of ſettled fine weather, except towards the cloſe of ſummer and the beginning of autumn, and even then we are frequently balked in our expectations of fine weather. The ſudden changes that take place during three fourths of our year

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may be regarded as no leſs prejudicial to the health, than diſagreeable to the feelings; and our terrors of *catching cold*, which have frequently appeared ridiculous to foreigners, are really better founded than we ourſelves are apt, moſt of us, to apprehend; colds, in their conſequences, proving fatal every year to multitudes both of the young and the old; to the former principally by giving riſe to conſumptions, to the latter by producing pulmonary diſeaſes of a different character.

We cannot hope entirely to eſcape the unpleaſant ſenſations, or to ward off the fatal effects, occaſioned by this caprice of our climate. But by underſtanding how colds are caught, we may be induced to take certain ſimple, but uſeful precautions. One may with the greater propriety embrace an opportunity of diſſeminating this ſort of information, as the manner in which colds or inflammatory catarrhs is taken, though now in my opinion perfectly aſcertained, is far from being generally underſtood by the members

members of the medical profeſſion; and if any perſon, not belonging to that profeſſion, ſhould ſuſpect this to be a wanton paradoxical aſſertion, he may find in the caſe of opium, and of the cool treatment of ſmall pox, &c. inſtances equally ſtriking, where one generation of pathologiſts paſſed away after another, without being able, in the caſe of opium *, to perceive the plaineſt appearances, or, in that of ſmall-pox, to draw the ſimpleſt concluſion. So ſervilely imitative an animal is man! ſo loath to employ his own powers of perception and thought!

* It is curious to ſee what pains medical writers have taken to imagine hypotheſes, either out of mere complaiſance to the term, *narcotic*, or becauſe opium is a drug in Chriſtendom, and wine an article of diet, rather than ſuffer themſelves to ſee that opium makes a man merry or drunk, then lays him aſleep, and afterwards cauſes him to awake with an head-ache, in the ſame manner as wine. One cannot compare Haller's clear and ſatisfactory parallel of wine and opium, publiſhed in 1769 (*El. Phyſiolog. t. V. p.* 610—11) with Cullen's perplexed and hypothetical doctrine of opium, and his whole article *ſedantia*, publiſhed in 1789, (*Mat. Medica, t. II.* 217, *& ſeq.*) without a ſenſe of humiliation.

When any part of the body has been expoſed to cold, it is liable to be much more affected by heat and other ſtimuli than before the expoſure. Of this the method of treating frozen limbs in cold countries affords a beautiful and deciſive proof. Were a frozen limb to be brought before a fire, or immerſed in warm water, a violent inflammation would come on, and ſpeedily terminate in mortification. They therefore take ſnow to rub the parts benumbed with cold, and very gradually expoſe them to a warm temperature. The glow, after coming out of the warm bath, is entirely owing to the lower temperature to which the body has immediately before been expoſed, or, what amounts to the ſame thing, the power of water to conduct heat away from the body faſter than air. The pungent pains felt upon holding an hand much chilled to the fire is another exemplification of the ſame principle, which ſeems to be one of the moſt general laws of animal nature. In like manner the ceſſation of action and thought during

ſleep

ſleep accumulates a power of thinking and acting with more energy than before we fell aſleep. Even Dr. Cullen, whom the prejudices of old age and the pride of celebrity conſpired to hinder from receiving this doctrine in its full extent, in his laſt work expreſſes himſelf upon the article of ſleep with a preciſion that is not always found in the theoretical part of his writings; 'a ſtate of 'ſleep,' ſays he, 'ſubſiſting for ſome time, 'induces a ſtate of the ſyſtem *more ready* '*to be affected by ſtimuli of all kinds**.' The latter part of the ſentence is a very accurate and luminous interpretation of the common expreſſion, *that we are refreſhed by ſleep.*

* *Materia Medica, II.* 228. In his little book on phyſiology ſome ingenious hints towards this doctrine will be found: Brown doubtleſs profited by theſe hints, and, I fear, without due acknowledgment; the old Profeſſor, on the contrary, who had gone into other theories, which he outlived, could not bear to think that a man, who had been almoſt his ſervant, ſhould have matured his ideas into a ſyſtem highly ingenious and partly juſt.

Now

Now after the application of cold, which, according to circumſtances, produces a greater or ſmaller diminution of the actions of the living ſyſtem, and at length ſleep itſelf, there may be an infinite number of gradations between a fatal inflammation and a tranſitory glow, and this according as the previous cold and the ſubſequent heat have varied in intenſity; but whatever be the degree, the effect depends on the ſame principle. By reſpiring a cold atmoſphere the ſame thing happens to the noſtrils, fauces, lungs, as to the external ſurface of the body upon going into a cold bath; and if we paſs ſuddenly from ſuch an atmoſphere into a warm room, what happens to the ſkin will in ſome degree happen to the membrane lining theſe cavities; a glow or inflammation will enſue, according to the difference between the two temperatures and the length of time paſſed in the cold. When the application of cold or moiſture to a ſuperficial part only is ſucceeded by an inflammation of the reſpiratory cavities, the conſent of the whole ſyſtem

ſyſtem eaſily explains this remote local affection. The cauſe of diſeaſe pervades at once and feels as it were, or ſearches the whole body, but affects only in a degree to draw our notice the organ which from habit or ſtructure is moſt tender. Should any other part, from previous circumſtances, have been rendered more ſenſible to its influence, we ſhall in conſequence have a ſore throat, a diarrhœa, or the rheumatiſm, in place of a catarrh. Children are ſo ſuſceptible of inflammations that a great part of the mortality among them is, as far as I have obſerved and can judge, to be aſcribed to the ignorance of mothers and nurſes of the power which even a moderate change of temperature, if ſuddenly made, has to affect their tender and irritable frame. Whenever accurate regiſters of the mortality of the human ſpecies, in climates equably warm, ſhall be kept, I expect that not half ſo many infants will be found to die as in Britain. Hence, in part, the populouſneſs of ſuch countries: thoſe gardens of the earth

earth being equally calculated to rear and ſupport inhabitants.

In catarrh therefore, whatever be the degree of inflammation, from the ſlighteſt affection of the noſtrils or cheſt to the moſt acute pleuriſy, it never takes place, unleſs we paſs too ſuddenly into a comparatively warm atmoſphere, or apply a ſtimulus equivalent to heat. The feveriſhneſs, the internal glow, the dryneſs of the noſtrils, the *huſkineſs* of the bronchiæ, are never felt while we remain in the cold; they ſpeedily come on after entering a warm room. Perſons who can recollect their paſt, or will attend to their future ſenſations, will eaſily be convinced of the truth of this ſimple and ſalutary theory. Formerly, when I firſt heard it, I muſt own that I durſt not believe it on account of its ſimplicity *. It was

* As far as I can judge from recollection, the following latent hypotheſis was a great cauſe of doubt. Anatomy ſeemed to demonſtrate a wonderfully complicated ſtructure in the human body. It was obvious to imagine that the functions or uſe of this complicated ſtructure muſt

was not till having been made more attentive to facts by a desire to determine the merits of the different theories of catarrh, that I was irresistably convinced by my own personal experience of the justness of that shortly stated above, and which indeed requires but very few words for its exposition. Frequently after riding for hours in the rain, especially during summer, I have felt a glow infinitely more vivid than in coming out of the cold bath. This glow was owing partly to the temperature of the atmosphere, and partly to the slight exercise of changing cloaths. I have sometimes made other persons attentive to the progress of the phænomena, and nothing has appeared more evident than that during exposure to wet or

must be complicated in proportion. This is a false conception: the living body is in reality infinitely more simple than it appears at first view, at least as to the actions and diseases of its parts; and the structure is but a constant repetition of nerves, blood-vessels, cellular substance, fibres, and fat. The anatomical knowledge of a surgeon, indeed, must be minute, but it is not abstruse; not much more so than that of geography. It is perfectly analogous to subterraneous geography.

cold,

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cold, no tendency to inflammation is perceptible, but that ſubſequent heat, exerciſe in the dry, and ſtimulants, produce the glow or inflammation. By keeping quiet and cool for ſome time after being wet in ſummer, and by avoiding a ſudden tranſition into a warm temperature in cold weather, and by temperance in both caſes, thoſe inflammatory diſeaſes, for which cold only prepares the ſyſtem, may be eaſily avoided; and any perſon, by acting upon theſe principles, may have at pleaſure a ſlight or a violent catarrh, or no catarrh at all.

The popular treatment, therefore, of colds during their early ſtage is juſt as prejudicial, as the antient hot regimen during the ſmall-pox. Warm or ſpirituous liquors, warm cloſe rooms, and a weight of bed-clothes cannot but aggravate the ſymptoms. The ſame may be ſaid of Mudge's inhaler, which though certainly ſerviceable in the aſthenic catarrh, or *catarrhus ſenilis*, I have oftener than once obſerved ſenſibly to heighten

heighten the inflammation at the commencement of a common cold *.

It

* *Catarrhum igitur e frigore esse, calore solvendum, gravissimus error est. Contrà, frigus nunquam nocet, nisi ubi ejus actionem calor excipit.——Catarrhus æstate toties incidens, ubi sexcenties causa ejus a frigore supra repeti nequit, a calore potest; contagiosus nunquam, communis sæpe; frigoris egens; non omnino frigori, calori protinus succedens;——observationem eandem firmant.*

Such is the doctrine of Brown, (Elem. Medicin. II. 42, 43) a *noted* author and teacher, as he is stiled by Cullen, (Mat. M. II. 235.) who was very desirous to persuade himself that the adversary whom he notes, and against whom he protests, was contemptible. For my part, I consider his doctrine of the effect of stimulating powers applied to accumulated excitability as the only specimen of extensive reasoning in pathology, calculated to afford any satisfaction to a just thinker. He avoids those unmeaning or vague terms, that had been before so much employed to shelter ignorance from their employers and from others; he appeals to phænomena of the living system obvious to the senses; and adopts such principles of reasoning that, if he has not always discovered the truth, he is seldom forsaken by the spirit of a philosopher.

I beg leave to add—not for the sake of propitiating medical orthodoxy, but of preventing misapprehension—that in his system of opinions and practice, as delivered either in his lectures or writings, I have always found full as much

It has been, I believe, unfortunate for the inhabitants of this country, that we are not ſubject much to reject as to receive. His directions for the application of ſtimuli often ſeemed contrary to his principles; and, I believe, he led his followers into dangerous miſtakes on this important point. I have nothing to ſay in defence of his perſonal conduct, which more than any thing elſe cauſed his doctrines to be condemned unheard, as if the groſſneſs of a man's manners affected the concluſiveneſs of his arguments. If his imprudence, however, was highly blameable, and his arrogance intolerable, the liberal will allow ſomething to the deep conſciouſneſs of neglected merit, and to the irritating ſenſations attendant upon declining health: and poſterity may reproach an age, in which a man, poſſeſſed of powers ſo ſuperior and ſo nobly exerted, was browbeaten, defamed, almoſt perſecuted, and left to periſh in extreme penury.

It is to be lamented that Dr. Brown's writings ſhould be ſo little known to thoſe who are ſecretly influenced by his opinions, which in one way or other have been ſo widely diffuſed as to affect almoſt the whole practice of medicine in Great Britain. His miſtakes a diſpaſſionate man would eaſily diſtinguiſh and avoid; but as he certainly did not err wilfully, I know not why they ſhould be held up as criminal, or cenſured with greater aſperity than the exploded theories of Boerhaave or Cullen.

From converſation with the author of the *Botanic Garden*,

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ſubject to ſuch a continued ſeverity of cold, as ſhould oblige us regularly to fortify ourſelves by warm cloathing. By linen, worn excluſively, we loſe more in health than we gain in comfort; which comfort is, perhaps, after all, merely imaginary; for there is hardly an inſtance in which the ſkin does not ſoon reconcile itſelf to woollen, though there is no neceſſity for placing it next the ſkin, and cleanlineſs is juſt as much in the power of the wearers of woollen. The moſt ſimple method, as well as the

Garden, who is no leſs eminent as a phyſician than a poet, I find that he entertains ſimilar ſentiments concerning the Brunonian doctrine. Several of Dr. D.'s friends have indeed aſſured me, that he had diſcovered theſe principles many years ago, and reduced them to a regular ſyſtem. Indications of ſuch a ſyſtem appear in ſome of the notes to his beautiful poem, but the compleat treatiſe has never got beyond the circle of his friends; a rare example of modeſty or indifference to fame! It is much to be wiſhed that the anticipation may not finally deprive us of the treatiſe: ſome peculiar illuſtrations it muſt have, every one of which would enlighten the philoſopher and guide the phyſician. The authority of the writer too would ſecure the doctrines a fair hearing, and his ample experience would inſpire confidence in his reaſonings.

moſt effectual, to avoid the influence of ſudden changes of atmoſpherical temperature is to wrap the body in ſubſtances that conduct heat ſlowly. Both for this reaſon, and becauſe it is ſo much leſs unpleaſant, when moiſt, than linen, flannel ſhould be worn at leaſt above linen during every ſeaſon in Great Britain; and thoſe who feel it neceſſary may double it during the winter, ſpring, and beginning of ſummer.

In children it is of the utmoſt conſequence to keep the body cool, but never to ſuffer it to be cold. Thus, without being enervated, they may eſcape the fatal conſequences of heat ſucceeding quickly to cold; for it is not true, as ſeems, in conſequence of an analogy more or leſs diſtinctly conceived, to have been frequently imagined, that cold hardens children as it hardens ſteel.

Perſons advanced in years and ſubject to the aſthenic catarrh, ſuffer from the immediate application of cold or moiſture, which

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which is but cold in another form. The tranſition to a cold or damp air will immediately affect them: I have obſerved the ſymptoms to commence as ſoon as one breath of ſuch air was inhaled; and in ſome caſes they are very quickly freed from dyſpnœa, wheezing, defluxion, &c. upon removing into a warm dry air; inſomuch that there is a ſtate of the diſeaſe when a perſon may take and loſe this ſort of cold ſeveral times a day. I am afraid dreſs alone will never prove ſo effectual a preſervative againſt this as againſt the other kind of catarrh. If ſome portable apparatus for warming the air before its admiſſion into the lungs could be contrived, this perhaps would ward off thoſe pulmonic attacks, which though only dangerous at a certain age, or in a ſtate of conſiderable violence, are at all times diſtreſſing enough to be numbered among the innumerable evils of a moiſt and variable climate.

A very recent writer, by no means deficient in acuteneſs, queſtions, or rather ſhews

an inclination to queſtion, the baneful effects ariſing from the variableneſs of our climate. 'There is, perhaps,' he obſerves, 'no country where coughs are ſo frequent as in 'Great Britain, which is generally imputed 'to the exceſſive variableneſs of the climate. But this cauſe ſeems to have no 'ill effect upon the health of wild animals, 'who are expoſed to the ſame uncertain 'weather.'—This variableneſs 'does not 'prevent many vegetables and animals from 'ariving at their utmoſt perfection.

'The blooming complexion of our peaſantry—the permanence of their good 'looks, and their ſtrength and activity 'ſtrongly ſhew that the climate is not unwholeſome to thoſe properly educated. 'Strangers, however, often ſuffer by the ſudden tranſitions from heat to cold, and from 'dry to moiſt, which are ſo frequent. And 'thoſe natives ſuffer likewiſe who are delicately brought up, and who, from being 'generally confined in warm apartments, 'ſeldom feel the natural temperature of the 'air,

'air, and acquire in this northern region 'conſtitutions adapted to Italy.—They ac-'quire' (from change of weather) 'catarrhs, 'pleuriſies, conſumptions, ſore throats, fe-'vers, and other diſeaſes.

'But theſe ailments ought rather to be 'imputed to a delicate conſtitution, acquired 'by an improper mode of life, than to the 'climate.'—He goes on to ſay, that an infant born healthy, and hardily brought up, would be nearly as little liable to injury by the variableneſs of the weather as a young fox *.

Several reflections connected with the preceding obſervations are ſuggeſted by theſe paſſages. 1. If the wild and unhouſed animals are leſs ſubject to pulmonic diſeaſes, it is becauſe thoſe diſeaſes are rarely produced by natural heat; or by any ſtimuli, to which ſuch animals are expoſed. There cannot well be a more deciſive proof, than this general fact, of the truth of the theory. Upon

* Moore's Eſſay on the Materia Medica, 1792, p. 280, &c.

this principle we may account for an appearance that has often ſtruck the pathologiſt. The catalogue of the diſeaſes of wild animals is extremely ſhort. Domeſtic animals being leſs ſubject to the action of artificial heat and ſtimuli than man, but more ſo than wild animals, the effect is found in this inſtance alſo to be in proportion to the cauſe. Thus, as the author juſt quoted well obſerves, except a few domeſtic animals, ſuch as are ſtabled in warm buildings, or tended with human care, others are little liable to catarrh *. The moſt tender ſeem liable to few, except diſorders ariſing from the exceſs, or defect, or unwholeſome qualities of their food, and theſe are chiefly diſorders of the alimentary canal; by far the greater number would ſurvive to a good old age, if left to themſelves, and not, like half the human ſpecies in our climate, ſink back into the tomb, as ſoon almoſt as they have catched a few glimpſes of the light of heaven. 2. If delicate perſons are ſubject

* *Coughing*, the moſt equivocal of ſymptoms, may ariſe from a diſeaſed liver, &c. &c.

to diſeaſes and dangers in England to which they would not be ſubject in Italy, it is evident that the difference between the climates of England and Italy is the cauſe of theſe diſeaſes and dangers. 3. I am much afraid that the greateſt mortality of children is among thoſe hardily brought up. It is, I conceive, impoſſible for a man of humanity, or even of common reflection, to read the following paſſage without ſhuddering at the extreme wretchedneſs, and the wide extent of that wretchedneſs, which it announces. 'Poverty, though it does not prevent the generation, is extremely unfavourable to the 'rearing of children. The tender plant is 'produced, but in ſo cold a ſoil and ſo ſe'vere a climate, ſoon withers and dies. It 'is not uncommon, I have been frequently 'told, in the Highlands of Scotland, FOR 'A MOTHER WHO HAS BORNE TWENTY 'CHILDREN NOT TO HAVE TWO ALIVE*.' In England the children of the poor are ſwept away by hundreds and by thouſands;

* Smith's Wealth of Nations, I. 120.

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and the axiom of medicine, *penuria morborum cauſa*, is, I believe, even more ſtrikingly verified in the caſe of poor children than of their parents. But I ſuppoſe from what follows, that the author means by *hardily brought up*, ſuch as are not enervated. 4. I am much afraid, however, that the peaſantry, after weathering the early inclemencies of their ſtation, enjoy no ſuch advantage of freedom from pulmonic complaints as he ſeems to imagine. To mention one or two probabilities to the contrary: an inconſiderable city, and a thinly inhabited country, yield to the Oxford infirmary but a ſcanty ſupply of patients; yet I have been told, that for a great number of years paſt, there have been almoſt always among the number ſome ill of phthiſis, or threatened by that diſeaſe. Here it may be remarked, that neither confinement in cloſe rooms, unwholeſome fumes, or an unfavourable poſture, contribute any more than an effeminate education, to the production of conſumptions. Again, among the peaſantry of Warwickſhire and Staffordſhire, I under-

ſtand that conſumptions are extremely frequent; not leſs ſo than among the Birmingham manufacturers. And who has not often met with phthiſis in the moſt airy ſituations?

Among the richer claſs this fatal diſeaſe would, perhaps, be leſs common, did they not by a ſtrange infatuation take the moſt effectual ſteps to contract it. If a greater proportion of females fall victims to it, is it not becauſe, loſing ſight more than men of its primary purpoſe, they regulate their dreſs ſolely by fantaſtic ideas of elegance? If happily our regret ſhould recal the age of chivalry, to break the ſpell of Faſhion would be an atchievement worthy the moſt gallant of our future knights. Common ſenſe has always failed in the adventure; and our ladies, alas! are ſtill compelled, whenever the Enchantreſs waves her wand, to expoſe themſelves, half undreſſed, to the fogs and froſts of our iſland.

TWO

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TWO MEMOIRS

TRANSLATED FROM THE FRENCH

OF

DR. GIRTANNER *.

MEMOIR I.

THE irritable or contractile, improperly called the muſcular fibre, when ſeparated from the animal or the plant, preſerves its irritability for ſome time, and continues to contract upon the application of any ſtimulus. It retains alſo this property when it is cut into pieces, as is obſervable on dividing the proboſcis of a butterfly, or the ſtamina of plants. Each piece continues to contract, which proves that the ſmalleſt portion of the fibre poſſeſſes its

* N. B. Part of the firſt memoir is omitted.

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own peculiar irritability, independent of the other portions. The fluids in animals and vegetables are endued alſo with this property as well as the ſolids. Their irritability conſiſts in coagubility, which coagubility in fluids is ſubject to the ſame laws as the irritability of the fibre. This is a new diſcovery; and it forms the baſis of many important truths. The degree of irritability of both ſolids and fluids is conſtantly changing; it varies according to the age and habit of the ſame animal and the ſame plant, and according to the ſex, organization, and ſize of the different individuals. It is encreaſed by the abſence of the common and habitual ſtimuli, and is weakened by the too frequent application of the ſame ſtimulus, or by the application of a ſtimulus too powerful. We may diſtinguiſh three different ſtates of the irritable fibre, or three different degrees of irritability of which it is ſuſceptible.

1. The *ſtate of health*, which is peculiar to each individual, and which I ſhall call

call the *tone* of the fibre, after a term of Stahl.

2. The *ſtate of accumulation*, produced by the abſence of habitual ſtimuli.

3. The *ſtate of exhauſtion* produced by the too powerful operation of ſtimuli.

The *ſtate of health*, or the *tone* of the fibre, conſiſts in a certain quantity of the irritable principle neceſſary to its preſervation. To maintain this ſtate, the action of the ſtimulus muſt be ſtrong enough to carry off from the fibre the ſurplus of this irritable principle, which the lungs and the circulation of the fluids are continually ſupplying. For this a certain equilibrium is neceſſary between the ſtimuli applied and the irritability of the fibre; in fine, that the ſum of all the ſtimuli acting upon it may be always nearly equal, powerful enough to carry off from the fibre the exceſs of its irritability, and not ſo ſtrong as to carry off more than this exceſs. It is in this equilibrium

librium between the acting ſtimuli and the irritability furniſhed by the lungs and the circulation, that the *health* or *the tone* of the fibre conſiſts. When the ſum of the ſtimuli acting upon the fibre is not great enough to carry off all its exceſs of irritability, the irritable principle accumulates in the fibre, and then it is found in that ſtate which I call the *ſtate of accumulation*; the irritable principle accumulates in the fibre, its irritability is augmented, and the ſtimuli produce much ſtronger contractions than when the fibre only retains its tone. Hence it is that in oppoſing an obſtacle to the movements of the hedyſarum girans for any length of time, the movement becomes conſiderably ſtronger after the obſtacle is removed.

When the ſum of the ſtimuli acting upon the fibre is too great, the fibre is deprived not only of the exceſs of its irritability but alſo of ſome portion of the irritable principle neceſſary for the tone of the fibre, or, more properly ſpeaking, the fibre loſes more irritability

irritability than it receives, and of courſe in a ſhort time finds itſelf in a *ſtate of exhauſtion*, and this exhauſtion will be either *temporary* or *irreparable*.

In the ſtate of *temporary exhauſtion* the fibre loſes its tone, and fails for want of irritability. The application of a ſtimulus, while it is in this ſtate, will not make it contract. Provided the ſtimulus be not very ſtrong, it will produce no effect at all, but in a ſhort time the irritable principle will accumulate afreſh in the fibre, and then it will again contract. It is only by little and little that the fibre recovers its irritability. This truth, I dare venture to ſay, is as new as it is ſtriking. It unfolds a vaſt number of phænomena hitherto inexplicable. Let us obſerve, for example, the motion of the heart; the heart contracts from the ſtimulus of the blood, and impels the blood through the arteries; it then again dilates, and the blood enters. But the heart does not contract itſelf immediately upon the firſt impreſſion of the blood. Its irritability

lity having been leſſened by the preceding contraction, it requires half or three quarters of a ſecond before the irritability of the heart ſhall have accumulated to ſuch a degree that the new ſtimulus can act upon it. It is impoſſible to explain the motion of the heart upon any other principle. Haller has indeed very well explained the motion, on the principle of the irritability of the heart; but he was never able to anſwer the famous objection of his opponents, who ſaid, if the blood acts upon the heart as a ſtimulus, and its contraction is the conſequence of ſuch action, how comes it that the heart does not contract as ſoon as the blood enters it, but that it flows in ſome time before the contraction is renewed? Why does not the effect immediately follow the cauſe? Haller could never anſwer this objection, nor ſeveral others of the like nature, inaſmuch as he was a ſtranger to the laws of irritability. The menſtrual diſcharge in women is explained on the ſame principle. The ſtimulus of the ovaries acting continually in women after the age of puberty

puberty (as I ſhall prove elſewhere) nevertheleſs does not produce its effects till the end of eight and twenty days; becauſe this period of time is neceſſary for the uterus, in its ſtate of health, to accumulate its irritability in ſufficient quantity for the ſtimulus to act; the diſcharge ceaſes after the irritability of this organ has been diminiſhed and returns with the returning irritability. All the periodical motions in animals and plants, as well as their periodical diſeaſes, may be explained upon the ſame principle; that is to ſay, any ſtimulus which is always preſent, and continually acting upon the fibre, produces no ſenſible effect, till the exhauſted irritability of the fibre ſhall have been accumulated afreſh. The periodical motions in organized bodies depend on the alternate exhauſtion and accumulation of the irritability of the fibre. A temporary exhauſtion of the irritability of the hedyſarum girans is produced by the heat of the ſun and by electricity, according to the obſervations of M. Brouſſonet. The electrical fluid exhauſts alſo

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the irritability of the mimoſa pudica for ſome time, as the abbè Bertholon has obſerved.

The total or irreparable exhauſtion of the fibre conſiſts in a total loſs of its irritability, as in the caſe of gangrene. The fibre changes its colour, becomes dark or black, and ſubject to the laws of inorganized matter, and begins to decompoſe and putrefy.

A very powerful ſtimulus will in a very ſhort time reduce the fibre to this ſtate. Such, for inſtance, is the ſtate of the fibre in animals killed by very ſtrong poiſons, by the bite of a rattleſnake; in animals deſtroyed by a knife dipped in the juice of the aconite, or by poiſoned arrows. The irritability of many inſects, and of the greateſt part of plants, is irreparably exhauſted by the ſtimulus of the propagation of the ſpecies, ſo that they die the moment the work of generation is compleated. Dr. Prieſtly has obſerved, that in expoſing plants to the ſtimulus of air in which animal ſubſtances have

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have putrefied, it always happened that, either the plants being vigorous enough to bear the action of the ſtimulus, their growth was very rapid, or the ſtimulus proved too powerful, and the plants died; their irritability was exhauſted in an inſtant, and their leaves became black and gangrenous.

The irritable fibre, from the firſt moment of its exiſtence to that of its diſſolution, being conſtantly ſurrounded by the body which acts upon it, and ſtimulates it, and upon which it re-acts by its contraction, it follows, that during the period of its exiſtence the irritable fibre is in continual action; that its exiſtence conſiſts in action; and that it is not a paſſive ſtate, as ſome authors have aſſerted. Hence, external objects having no immediate action upon the nerves, and only acting upon them, and producing their different ſenſations, through the medium of the irritable fibre, it is plain that the ideas we have of external objects are not conformable to thoſe objects, but that they are varied and modified

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by the irritable fibre through which they are tranſmitted to us. Objects, therefore, appear different according to the different ſtates of the fibre. The irritable fibres, which are combined together in every individual, whether animal or vegetable, form a ſyſtem of fibres, in which the integral parts act continually upon the whole, while the whole re-acts upon the parts, ſo that every ſtimulus which acts upon any fibre in the ſyſtem will deprive that part of its irritability; but this loſs will ſoon be repaired by the ſyſtem, and every fibre will furniſh, in proportion, ſome ſhare of its own irritability to ſupply the loſs in any one fibre. Thus it is that a very weak ſtimulus, but one that is conſtantly acting upon one part of the ſyſtem, ſuch as ſlow poiſons, the abuſe of ſpirituous liquors, &c. exhauſts in the end the whole ſyſtem, and produces death. For the ſame reaſon, a very powerful ſtimulus applied to one part of the ſyſtem, ſuch as laurel water, opium, the poiſon of the rattleſnake, will in an inſtant exhauſt all the irritability of the ſyſtem, deſtroy

deſtroy the animal, and leave the fibres without any irritability. I am convinced, from repeated experiments, that opium, alcohol, ammoniac, a ſolution of ſugar of lead, ſulphuric æther, deſtroy animals by exhauſting the irritability of the whole ſyſtem, and that the muſcles of the animals deſtroyed have, by the application of theſe ſtimuli, been wholly deprived of their irritability. The effect was the ſame when theſe ſtimuli were applied to the muſcles and ſtomach, and when injected into the veins of animals. I have alſo made very curious experiments with the ſame ſubſtances upon vegetables.

The irritable fibres in the ſame ſyſtem have not all the ſame degree of irritability. They have different degrees of *capacity* for the irritable principle. The capacity of the fibres is in the ratio of their diſtance from the heart. Thoſe equally diſtant have the ſame capacity. Every ſtimulus which affects one of the fibres affects the others at the ſame time and in like manner. Hence

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the ſympathy of different and ſeparate parts and thoſe ſurpriſing phænomena which hitherto have been explained by the harmony of the nerves, although we ſee the ſame phænomena in the vegetable kingdom, which is deprived of nerves. Theſe ſympathetic phænomena are obſervable throughout organized nature. Whatever part of the polypus be touched, the whole will contract, and its arms will contract themſelves by ſympathy. If a worm be touched with the point of a pin, without wounding it, the whole worm will be ſeen to contract itſelf; which is a certain proof that the differents parts are affected by ſympathy. If the ſlighteſt impreſſion be made upon one of the leaves of the averrhoa carambola, not only that leaf but all the neighbouring ones, and frequently ſome of the diſtant ones, will contract themſelves by ſympathy.

When the irritable fibre has loſt its tone, and fails, either from an exceſs of the irritable principle, or from a deficiency of this

this principle, it is diſeaſed, and the ſyſtem of which it forms a part ſuffers and becomes diſeaſed through ſympathy. All the diſeaſes of animals and vegetables may be ranged under two heads; to wit: Firſt, the *diſeaſes of accumulation* cauſed by the accumulation of the irritable principle, and by the diminiſhed action of the habitual ſtimuli. Secondly, the *diſeaſes of exhauſtion*, cauſed by a defect of the irritable principle proceeding from the encreaſed action of the habitual ſtimuli, or from the addition of new ſtimuli. Under theſe two claſſes may be ranged all diſeaſes whatever. Paradoxical as this propoſition muſt neceſſarily appear to thoſe who have not reflected on the ſubject, it is nevertheleſs true, and I ſhall give the moſt convincing proofs of it in a work I am about to publiſh.

Remedies remove the diſeaſe by their action upon the irritable fibre, and by exhauſting its irritability, when the diſeaſe is that of accumulation; or by diminiſhing the action of the common ſtimuli, and conſe-

quently by preventing a total exhauſtion, where the diſeaſe is that of exhauſtion. The effects of poiſons are to be explained in this way.

Poiſons, remedies, and in general all ſurrounding bodies acting only on the irritable fibre, it follows that they act upon the ſyſtem in a ſimilar manner, and that every ſubſtance capable of producing the greateſt poſſible effect upon the fibre, that is to ſay, every ſubſtance capable of exhauſting all the irritability both of the fibre itſelf and of the ſyſtem in an inſtant, as, for inſtance, laurel water, or white arſenic, is alſo capable of producing all the inferior degrees of action, either by acting upon a fibre leſs irritable, or by acting upon the ſame fibre, but in a leſs quantity. Laurel water, opium, white arſenic, ammoniac, are of courſe both medicines and poiſons capable of healing as well as of producing all maladies whatſoever without exception. And this is confirmed by a number of experiments which I have made upon different animals. This truth ſeems to me of the utmoſt importance;

portance; and the abbè Fontana, who made more than ſix hundred experiments to prove that ammoniac is no remedy againſt the bite of the viper, would have ſaved himſelf a great deal of time and trouble, had he been acquainted with it. If inſtead of applying the venom of the viper to ſo many animals, and afterwards applying ammoniac to the wound, he had made a ſingle comparative experiment, and had applied ammoniac to a wound made by a lancet that was not poiſoned, he would have found that ammoniac itſelf, applied in this manner, would have produced a diſeaſe exactly analogous to that cauſed by the venom of the viper, and, conſequently, ſo far from removing the malady, muſt neceſſarily increaſe it, by exhauſting the irritability of the fibre in a much leſs time than the venom of the viper by itſelf was capable of doing. Mr. Fontana has made more than ſix thouſand experiments upon the poiſon of the viper; he employed more than three thouſand vipers, and cauſed to be bit more than four thouſand animals, and the concluſion he

drew

drew after this truly enormous number of obſervations was, that the poiſon of the viper kills all animals, and produces the diſeaſe by its action on the blood. But why did Mr. Fontana neglect to make the deciſive experiment, the *experimentum crucis* of Bacon. It is well known that frogs, and many animals with cold blood, live a long time without the heart, and entirely deprived of blood. If therefore the poiſon of the viper kills animals by its action on the blood, it will not deſtroy frogs without blood. But experiment contradicts this reaſoning. The poiſon of the viper will kill frogs without blood in as ſhort a time as it kills thoſe animals who have not loſt their blood. It is not therefore by its action upon the blood that the venom of the viper deſtroys animals; and thus does it happen that a ſingle experiment frequently overturns all that ſix thouſand other experiments have apparently eſtabliſhed. According to my experiments, poiſons operate upon the blood juſt as they do upon the muſcular fibre, by depriving it of its principle

ciple of irritability, or of its oxygene. After having made this obſervation upon the experiments of Mr. Fontana, I muſt do him the juſtice to add, that I have found all his experiments very accurate, and that in all thoſe which I have repeated, the reſult has been exactly conformable to the account given by him; it is in his concluſions only that he appears to be deceived.

The effect produced upon the irritable fibre by any ſtimulus, is in a ratio compounded of the degree of irritability of the fibre, and of the force of the ſtimulus. The ſame ſtimulus will produce greater contractions upon a fibre more irritable than upon one leſs irritable; and the irritability of the fibre being the ſame, it will contract itſelf more upon the application of a ſtronger than of a weaker ſtimulus.

The effect produced upon an irritable fibre by any ſtimulus is in the inverſe ratio of the repetition of its application. Cæteris paribus, the effect of any ſtimulus diminiſhes

miniſhes every time its application is repeated, till at laſt the effect is nothing, or $= o$. This explains the phænomena of habit, and many other phænomena hitherto inexplicable in the animal and vegetable œconomy. The mimoſa pudica, for example, expoſed to a ſtrong wind, contracts itſelf; but it ceaſes to contract itſelf in obedience to this ſtimulus after it has been accuſtomed to it.

The effect produced upon the irritable fibre by any ſtimulus, is in a ratio compounded of the degree of irritability of the fibre and the degree of the force of the ſtimulus directly, and the degree of the habit of the fibre inverſely. Let the force or intenſity of the ſtimulus $= a$, the degree of irritability of the fibre $= b$, the degree of the habit of the fibre $= c$, then the effect produced upon the fibre or x will $= \frac{ab}{c}$. But all the ſtimuli acting in the ſame manner, that which diminiſhes the irritability of the fibre for a certain ſtimulus,

lus, will in the like manner diminiſh it for the ſtimulating force in general, wherefore the habit of the fibre is comprehended under its degree of irritability, or *c* is comprehended under *b*. Therefore *x* will = *ab*.

The effect produced upon the irritable fibre by any ſtimulus, or *x*, being always equal to *ab*, it follows that the value of *a* and *b* being known, the value of *x* is known. But admitting an unity fixed and conſtant, it will be eaſy in all caſes to expreſs by numbers the degree of irritability of the fibre and the degree of the force of the ſtimulus, or the value of *a* and *b*, conſequently it will be eaſy to find the value of *x*. All the art of medicine then conſiſts in the art of finding the value of *x*, that is to ſay, in finding a ſtimulus adequate to reſtore the tone of the fibre. Thus, if theſe principles be true, phyſic, which at preſent is an art of mere conjecture, will be reduced in time to the certainty of calculation, and after tables ſhall be formed to expreſs the values

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values of *a* and *b*, and the ſigns by which they may be known, this calculation will be ſo ſimple and eaſy, that it will form a part of the education of every individual. But further, the irritable fibre being the ſame in all organized nature, diſeaſes and their remedies will of courſe be the ſame for all organized beings: there will then be no diſtinction between medicine, farriery, and agriculture, but all theſe ſciences will be confounded, and become one, under the general name of *univerſal phyſiology*. The art of pharmacy and the ſcience of preſcription-writing will become uſeleſs; a phial of alcohol or laudanum will ſupply the place of that enormous quantity of drugs which crowd the ſhops of apothecaries. The trade of the druggiſt—but hold, if I continue this prophetic language, I ſhall only expoſe myſelf to ridicule; for, as Helvetius obſerves, 'every idea very foreign to our habits of ſeeing and thinking appears ridiculous to us. We never value any ideas but what are analogous to our own, becauſe we are under the neceſſity of eſteeming ourſelves only in others.'

Thoſe

Thoſe ſtimuli which I call common and habitual, becauſe they act continually more or leſs upon the irritable fibre, are, *heat*, *light*, *nouriſhment*, *air*, *the circulation of the blood*, *the ſtimulus of generation*, and *the nervous ſtimuli*. So long as the action of theſe ſtimuli is in proportion to the degree of irritability of the ſyſtem, and the ſum of their action is nearly equal to the ſum of the irritable principle abſorbed by the lungs, and diſtributed by the circulation, the whole ſyſtem will be in proper order, and the conſtituting fibres will have their tone. When one or more of theſe ſtimuli act more powerfully than ordinary, or the fibre becomes more irritable, while the degree of the action of the ſtimuli remains the ſame, the *exhauſtion* of the ſyſtem, and one of the diſeaſes in its train, will be the conſequence.

The abſence of one or more of theſe ſtimuli will produce an *accumulation* of irritability in the ſyſtem, and give birth to one of the diſeaſes of this claſs. I ſhall ſpeak of

of all theſe ſtimuli ſeparately, in order that I may be better able to explain myſelf.

Of heat.—The heat of the atmoſphere, and of all ſurrounding bodies, acts upon the fibre and ſtimulates it. I am convinced of the ſtimulating action of heat from direct experiments. I have expoſed ſmall animals, ſuch as cats, dogs, rabbits, &c. in covered veſſels, to the heat of boiling water, which ſurrounded the veſſel in which the animal was placed, ſo that the water could not touch it. Animals deſtroyed by heat in theſe experiments, upon diſſection have been found to have loſt all their irritability. Their heart and muſcles contracted themſelves but feebly, even upon the application of the ſtrongeſt ſtimuli, ſuch as electricity. It is proved by ſome beautiful experiments of Mr. Hope, that heat acts as a ſtimulus upon plants; and it is obſervable that plants expoſed to the ſun are larger, and produce more flowers and fruit than thoſe which are leſs expoſed to heat. Trees in general are more luxuriant which grow

grow in the ſouth than thoſe in the north. This is a proof that heat is a ſtimulus to the irritable fibre. The diſeaſes of hot climates are all the diſeaſes of exhauſtion, cauſed by the too powerful action of the ſtimulus of heat. Hence the cuſtom of taking ice in hot countries to reſtore the tone to the fibre, by abſorbing the heat and preventing its ſtimulating action. The irritability of the hedyſarum gyrans is exhauſted by the heat of the noonday ſun, according to the obſervations of M. Brouſſonnet; and by the experiments of M. Fontana and M. Medicus it is proved that the irritability of plants is great in the morning, diminiſhed during the heat of the day, and little or none in the evening.

Of cold.—Cold being a leſs degree of heat, its effects upon the irritable fibre are in proportion to the *habit*, or the quantity which is neceſſary to the fibre to preſerve its tone. The animals and plants of hot climates, that require the ſtimulus of a great heat to preſerve the tone of their leſs irritable fibres,

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are affected by the leaſt abſtraction of this habitual ſtimulus; the irritability of their fibres accumulates in conſequence of this abſtraction, and the return of the heat again exhauſts the fibre. The more intenſe the cold is, the greater is the accumulation of irritability. After the fibre has been expoſed for ſome time to a great degree of cold, its irritability is encreaſed to ſuch a degree, that the moſt trifling degree of heat produces the moſt violent effects: hence the glow experienced in coming out of a cold bath; hence the diſeaſes which are caught in coming out of the cold air into a warm room, and which medical men attribute to checked perſpiration, a ſuppoſition entirely falſe.

The leaſt movement is attended with fatigue upon the ſummit of high mountains, as I have frequently experienced, but eſpecially in 1785, upon the top of the Buet, and, as M. Sauſſure has likewiſe obſerved, upon the ſummit of Mount Blanc. The reaſon of it is this; the fibre is rendered ſo irritable by

by the cold of thoſe mountains, that the leaſt motion of the muſcles, or what is the ſame thing, the leaſt action of the nervous ſtimuli, exhauſts it. It is by the gradual application only of heat that frozen limbs can be recovered, and it is neceſſary always to begin by rubbing them with ſnow; without this the fibre will be exhauſted, and become gangrenous. During the winter, by the abſence of the ſtimulus of heat, and in part of light, plants and many animals become torpid, the organs of circulation and of nutrition perform their functions but languidly, and life itſelf appears ſuſpended. In conſequence of the diminiſhed action of theſe ſtimuli, the irritability accumulates, and manifeſts itſelf at the return of ſpring. The leaſt degree of heat then produces the moſt violent effects upon the fibres thus delicately irritable. Animals, which had concealed themſelves under ground, venture forth from their ſubterraneous retreats, plants put forth their leaves and flowers, and man himſelf is ſenſible of the ſtimulus of heat in the gales of ſpring,

his fibre being rendered more irritable by the winter's cold. Vegetation is much more vigorous in ſpring time than during the reſt of the year. It diminiſhes during ſummer in proportion as the irritability accumulated during winter is diminiſhed by the action of heat and light, and, laſtly, is exhauſted in the autumn. Dr. Hales obſerved, that the rapidity with which the ſap circulates in the vine during ſpring is five times greater that the rapidity with which the blood flows in the arteries of a horſe. This motion is much ſlower in ſummer, and almoſt ceaſes in autumn. It is not the effect of the heat alone, for if that were the caſe it would increaſe as the heat increaſed, and the effect would be proportionate to the cauſe; it is the effect of the irritability accumulated in conſequence of the abſence of heat during the winter. The effects of winter are very great in cold climates, becauſe the accumulation of the irritability is in proportion to the abſtraction of the ſtimulus of heat. In Lapland corn ripens in ſixty days, whereas in France it requires

an

an hundred and twenty, or an hundred and thirty days. The truth of what is here advanced may be proved by expoſing vegetables alternately to heat and cold: it is ſurprizing how much their growth and the power of vegetation is increaſed. But in theſe experiments care muſt be taken to vary the temperature by degrees; becauſe the irritability accumulating in the fibre by the abſtraction of the heat, a very ſmall quantity of this ſtimulus then applied is ſufficient to exhauſt it entirely, or to deſtroy it. Hence it is that the return of cold and froſt in the beginning of ſpring is ſo noxious to vegetables, and that the year is in general more abundant after a very cold winter. Mr. Fontana obſerved, that during winter the vipers which he kept for his experiments were in a torpid ſtate, though the thermometer was at 59°. He endeavoured to render them vigorous by warmth, and expoſed them to a heat of 67° only. In two minutes they died, though during ſummer they bear a much greater degree of heat; but then they are leſs irritable. Spallanzani obſerved

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that newts bury themſelves in the earth, and become torpid, in the month of October, before the thermometer in the ſhade falls to 54½, and that they re-appear in the month of February, though at that time it freezes every night, and frequently during the day the thermometer is many degrees below 54°. What is the reaſon, enquires this excellent obſerver, that theſe animals revive in ſpring, when the cold is more intenſe, and ſink into torpidity at a much leſs degree of cold in autumn? I will ſolve this problem, by obſerving that in autumn a very great ſtimulus is required to act upon the fibre of theſe animals, exhauſted as it has been by the heat of the ſummer; but in ſpring, the leaſt ſtimulus, the leaſt increaſe of heat, is ſufficient to put the fibre in action, its irritability having accumulated during winter in conſequence of the abſence of the common ſtimuli.

Light is another common ſtimulus. To convince myſelf of the ſtimulating quality of light upon plants by direct experiments, I enveloped

enveloped the leaves of ſome plants in an opake body, ſo that the air might have free acceſs, while the light could not penetrate. I found that theſe leaves became more irritable than the others, the irritability having accumulated. By the abſtraction of the ſtimulus of light, the irritability of organized bodies accumulates, and a diſeaſe enſues, which is called *étiolement*. Animals deprived of light, and living in dark places, loſe their colour and become white, as is obſervable in arctic animals during the long nights in the countries near the pole: I have obſerved it alſo in the animals that inhabit the Alps, and which conceal themſelves for the greateſt part of the year in ſubterraneous dwellings. *Blanched* plants loſe their green colour, and become whitiſh and ſickly. Some poiſonous plants loſe their noxious qualities, and become agreeable to the taſte, merely by the abſtraction of the ſtimulus of light. White animals and plants are very irritable; and it is obſerved that theſe animals and plants are not capable of ſupporting a great quantity of light.

The action of the light upon plants has been very well obſerved by Dr. Ingenhouſz and Mr. Senebier, and the manner in which colours are produced has been explained by M. de la Metherie. It is well known that animals that have been tamed, and eſpecially domeſtic animals, change their colour by education; but an obſervation that has perhaps eſcaped naturaliſts is, that this change is conſtantly from dull colours to thoſe that are brighter or leſs dull. I have often obſerved, that the change takes place more frequently in dark than in light places. Mice kept in a cage in a dark room have produced white mice.

The third common ſtimulus *is that of nutriment*. It requires a very ſmall quantity to ſupply the daily loſſes; the greateſt portion is employed in depriving the ſtomach, and of courſe the whole ſyſtem, of its ſuperfluous irritability. This is proved by what is obſerved in organized bodies. All animals are more irritable before than after food. Hunger, of which appetite is the leaſt

leaſt degree, is cauſed by the accumulated irritability of the ſyſtem. The gaſtric juice acts upon the fibres of the ſtomach become more irritable, and produces the ſenſation of hunger. Spallanzani has obſerved that birds of prey do not void indigeſtible bodies, ſuch as pieces of glaſs or metal, which they have taken in with their food, before their ſtomach is empty. Theſe indigeſtible bodies cannot be voided while the ſtimulus of the nutriment acts upon the ſtomach; but as the abſtraction of this ſtimulus gives the irritability of the ſtomach an opportunity of accumulating, the indigeſtible bodies very ſtrongly ſtimulate the fibres of the ſtomach, make them contract, and by this contraction they are voided. It is poſſible to do almoſt wholly without nutriment, by applying from time to time ſome other ſtimulus to the ſtomach, ſuch as tea, coffee, alcohol, opium, and by exhauſting by theſe means the accumulated irritability of that organ. By the entire abſtraction of the ſtimulus of nutriment, the

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the irritability of the ſyſtem is prodigiouſly increaſed. There are many inſtances of perſons who, not having eaten any thing for many days, have been intoxicated, and killed, in conſequence of ſwallowing, with great greedineſs, two or three cups of broth. Plants ſuddenly tranſplanted from a meagre, into a very rich ſoil, produce no fruits or ſeeds, and die in a ſhort time of a particular diſeaſe, cauſed by exceſs of nutriment.

The *circulation of the fluids* is the moſt powerful of the common ſtimuli. The blood, which oxygenates itſelf during its paſſage through the lungs, parts with its oxygene in the circulation, the oxygene having a ſtronger attraction for the irritable fibre than for the carbon which is contained in the blood. In this operation the heat combined with the oxygene is ſet free. Hence animal and vegetable heat. The blood acts continually upon the irritable fibre, and the fibre re-acts upon the blood, and this action and re-action are ſtronger in

proportion as the circulation is more rapid, and as the air which comes in contact with the blood in the lungs contains more oxygene air.

When any local ſtimulus continues to act upon any part of the ſyſtem, the circulation becomes more rapid, and a fever is the conſequence. Is the ſtimulus weak, a ſlow fever enſues, which will by little and little exhauſt the irritability of the ſyſtem, and the patient will die of a conſumption. Is the ſtimulus ſtronger, or the fibre upon it acts more irritable, we ſhall have an ardent fever, which will exhauſt the irritability in a leſs time. In fine, is the ſtimulus very violent, or the fibre diſeaſed by an exceſs of irritability, we ſhall have a putrid fever, which will deſtroy the patient, whether animal or vegetable, and will exhauſt the irritability in a very ſhort time. But whatever be the nature of the fever, the fibre irritated by the ſtimulus will act upon the blood more than ordinarily,

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rily, the re-action of the blood will be increaſed in proportion, the circulation will be more rapid, the blood will abſorb more oxygene, and the whole ſyſtem will be ſurcharged. By this means the irritability will be increaſed, the animal heat augmented, and the effect of the action of the ſtimulus becoming greater in proportion to the accumulation of irritability, a total exhauſtion of the irritability, or the death of the patient, will enſue. There are two methods of preventing the fatal effects of a local ſtimulus, whoſe operation upon one part of the ſyſtem is conſtant. The firſt conſiſts in preventing the ſurcharge of oxygene in the blood, which is accompliſhed by diminiſhing the proportion of oxygene gas in the air breathed by the patient, or by diminiſhing the quantity of blood by phlebotomy. The ſecond method conſiſts in applying ſtimuli capable of exhauſting the irritability in proportion as it accumulates; ſuch as wine, opium, bark, heat, &c. Phlebotomy acts by diminiſhing the

the quantity of blood, and conſequently its operation is attended with this effect, viz. of diminiſhing the re-action, and reſtoring to the fibre its tone. I ſhall here obſerve by the way, that the advice which many phyſicians have given, to make the patient breathe oxygene gaz, is the moſt pernicious they can give; for the patient always finds himſelf worſe after having breathed this ſalutary gas, as I have frequently had occaſion to obſerve.

The *nervous ſtimulus* is the only one which is peculiar to animals. It is this ſtimulus which is the cauſe of the voluntary motions, of convulſions, and paſſions. The paſſions differ from one another only in ſtimulating the irritable fibre more or leſs. Anger and joy are very powerful degrees of the nervous ſtimulus; content and hope are weak degrees; fear, ſorrow, fright, deſpair, are not abſolute degrees of this ſtimulus, they are only the abſtraction of the ſtimuli of hope, content, and happineſs. Anger and joy

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joy act as very powerful ſtimuli, and exhauſt the irritability of the fibre in the ſame manner as any other ſtimulus whatever. Content and hope are degrees of the nervous ſtimulus, neceſſary to preſerve the tone of the fibre. Sorrow and fright are degrees too weak. If they continue to act, the irritability of the fibre accumulates. It is well known that fearful and melancholy perſons are oftener affected by the ſtimulus of contagious diſeaſes than they who are free from fear, and who take the precaution of applying a greater quantity of ſtimulus than ordinary to their fibres, by taking wine, vinegar, opium, and bark. According to the obſervations of Mr. Fontana, timid and fearful animals die much ſooner of the bite of the viper than courageous or irritated animals. Joy excited by the annunciation of good news to a ſorrowful perſon, and one of courſe very irritable, has often cauſed death. The ſtory of the Roman mother is well known, who was bewailing the death of her ſon, and who dropt

dropt down dead for joy the moment ſhe ſaw him enter her room alive.

By the abſtraction of many of the common ſtimuli for any length of time, the irritability of the fibre accumulates ſo much, that the moſt trifling ſtimulus produces the moſt violent effects, and frequently even inſtantaneous death. This diſeaſe is called the ſcurvy, concerning the nature of which medical men have formed ſo many falſe and ridiculous theories. It is of the utmoſt importance to mankind to know the true nature of this diſeaſe; ſince, in conſequence of our ignorance in this particular, we have been unable to find a ſure remedy for it, and ſo many thouſands of lives have fallen a ſacrifice to its ravages, in armies, fleets, and beſieged towns. In the laſt war the Engliſh fleet ſuffered dreadfully from the ſcurvy; and laſt year a great number of ſoldiers died of this diſeaſe in the imperial army in Walachia, in conſequence of the abſtraction of the ſtimulus of nutriment (the emperor having

having ordered that a kind of paſte made of bread and water ſhould be given to the ſoldiers inſtead of meat), of the ſtimulus of oxygene, in the corrupted atmoſphere of the fens of Walachia, and laſtly, of the nervous ſtimulus, the moſt powerful of all; for the greateſt part of the army were engaged by force, and againſt their wills.

The abſtraction of all theſe ſtimuli accumulated the irritability of the fibre, and cauſed the ſcurvy, and that dreadful mortality that took place in the army. The ſame cauſes produce the ſame effects upon animals. We ſee domeſtic animals affected with the ſcurvy in conſequence of cold and hunger, that is to ſay, in conſequence of the abſtraction of the ſtimuli of heat and nutriment.

The ſheep which captain Cook had on board his ſhip, on his voyage round the world in the years 1772, 3, and 4, died of the ſcurvy, their teeth fell out, their gums rotted;

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rotted; in a word, they had all the ſymptoms of an inveterate ſcurvy. The abſtraction of the common ſtimuli in plants produces ſimilar ſymptoms and a ſimilar diſeaſe. The diſeaſe of rye called *ergot* is exactly analogous to the ſcurvy in animals; the *ergot* is the ſcurvy of plants; it is the effect of accumulated irritability in the fibres of plants. The cauſes which produce the ergot of rye are the ſame as thoſe which produce the ſcurvy in animals. According to the obſervations of Saillant and Teſſier, theſe cauſes are, a wet and barren ſoil, and a cold ſummer; that is to ſay, the cauſes of the ergot are, the abſtraction of the ſtimuli of nutriment and heat. I could enlarge upon this intereſting ſubject, if I were not afraid of making this eſſay too long. I wiſhed to give only the outlines, or a general view of my theory, without entering into the detail. In the ſubſequent eſſays I ſhall treat of oxygene conſidered as the principle of irritability, of the compoſition and decompoſition of water in

animals and plants, of the different kinds of air contained in the interior cavities of organized bodies, and of the circulation of this air, the exiſtence of which has not hitherto been even ſuppoſed, although, as I ſhall prove hereafter, the lymphatics in animals, and the fibres in plants, are almoſt ſolely deſtined for the circulation of theſe elaſtic fluids.

MEMOIR

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MEMOIR II.

HAVING given this general ſketch of a new ſyſtem of phyſiology, founded upon a number of experiments ſhewing that irritability is the principle of life, I proceed to prove that oxygene is the principle of irritability; that it unites with the blood in the lungs during reſpiration; that it is diſtributed to every part of the ſyſtem by the circulation, and that it combines with ſtimulating ſubſtances, with which the different parts of the ſyſtem come in contact.

I think that the oxygene is abſorbed by the blood, and that the venous blood is oxygenated in the lungs during reſpiration. The moſt celebrated naturaliſts and chymiſts are of a different opinion; they think that the oxygene does not combine with the venous blood. According to them,

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this laſt loſes carbone and hydrogene, and recovers the bright colour natural to it, without abſorbing any thing from the atmoſphere.

Here are ſome experiments and reaſons, upon which their theory of reſpiration is founded.

1. Arterial blood expoſed to the contact of hydrogene air loſes its vermilion colour, and aſſumes the black and deep appearance of venous blood. The hydrogene air is abſorbed in part in this experiment.

2. Mr. Hamilton made three ligatures in the jugular vein of a cat. Having expelled the blood from between two of the ligatures, he then introduced the hydrogene air, and kept it there, cloſing the aperture through which he had introduced it. He then looſened the middle ligature, and the blood contained between that and the third ligature came into contact with the hydrogene

gene air. In about an hour, having taken the blood from the vein, he found it liquid, and of a colour almoſt as black as ink.

3. Venous blood expoſed to vital air acquires the vermilion colour of arterial blood, and the air is rendered impure.

Mr. Lavoiſier and Dr. Crawford (Annales de Chimie, tom. V. p. 267) have drawn the following concluſions from theſe experiments:

1. That the change of colour which the blood undergoes during circulation proceeds from its combination with hydrogene air.

2. That in paſſing through the lungs, the blood parts with a portion of the hydrogene it contains, and then re-aſſumes its vermilion colour.

Mr. Lavoiſier and Dr. Crawford think, that during reſpiration the vital air which

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is received into the lungs combines with the carbone and hydrogene that is diſengaged from the blood; that it forms carbonic air with carbon, and water with hydrogene; and that the blood recovers its vermilion colour after it has loſt the carbon and hydrogene with which it had been charged during the circulation.

Without preſuming to contradict philoſophers of ſuch diſtinguiſhed merit, I cannot help obſerving, that it appears to me that theſe concluſions do not neceſſarily follow from the experiments, and that they are to be explained in a manner more conformable to the laws eſtabliſhed by modern chemiſtry. I know no experiment which authorizes us to ſuppoſe that carbone can unite with oxygene in a temperature of 97°—99°, or that hydrogene and oxygene airs combine and form water in ſo low a temperature. M. Seguin has attempted to anſwer this objection, by ſuppoſing that the carbon is in a very attenuated ſtate in the blood, and by citing the experiments of

* M. Ber-

M. Berthollet upon hydrogene air. But this explanation appears to me hypothetical, and no way convincing.

After having a long time attended to the phænomena of reſpiration, and made many experiments upon this ſubject, I think it may be concluded, that during reſpiration one part of the oxygene of the vital air combines with the venous blood, of which it changes the black colour and makes it vermilion *; the ſecond part of the oxygene unites with the carbon contained in the carbonic-hydrogene gas, which exhales from the venous blood, and forms carbonic acid air; a third part of the oxygene unites with the carbon of the mucus, contained in great quantities in the lungs, and which is continually decompoſing; this part alſo forms carbonic acid air; a fourth part of the oxygene combines with the hydrogene of the blood to form water, which is exhaled during reſpiration. The

* Dr. Goodwyn had proved this before. Could Dr. Girtanner be unacquainted with his experiments?

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heat contained in the vital air being decompoſed, remains united in part with the oxygene and the blood. Hence the quantity of heat peculiar to the arterial blood, which is much greater than that of the venous blood. Another part of the heat enters into combination with the carbonic acid air. Laſtly, a third part produces a temperature neceſſary for the formation of water, by the combination of the hydrogene and oxygene airs.

The effects of reſpiration will conſequently be theſe:

1. The venous blood loſes the carbonic-hydrogene air which it contains, and abſorbs the oxygene air, which gives it its vermilion colour, ſuch as it gives to metallic oxids, nitrous acids, and many other ſubſtances with which it enters into combination.

2. The capacity of the blood will be increaſed, becauſe oxygene increaſes the capacity

capacity of all ſubſtances to which it unites.

3. The oxygene air of the atmoſphere is partly abſorbed by the venous blood, changed in part into carbonic acid air by the carbone of the blood and that of the mucus of the lungs, while the reſt forms water with the hydrogene air of the blood, and a quantity of heat is ſet free.

The products of reſpiration will be—

1. Animal oxid, fluid (i. e. arterial blood.)

2. Carbonic acid air.

3. Water.

4. A ſmall quantity of liberated heat.

Nothing is more eaſy than to explain by this theory the experiments above related.

If

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If we expoſe, under a veſſel filled with hydrogene air, arterial blood to the contact of this air, the quantity of air will be diminiſhed, loſe its vermilion colour, and become livid. In this experiment exactly the contrary takes place of that which is obſervable in reſpiration. The hydrogene air unites with the oxygene of the arterial blood to form water, and the arterial blood, being deprived of its oxygene, becomes black, and is changed into venous blood; the deep colour which it aſſumes proceeds from the loſs of its oxygene alone. The experiment of Mr. Hamilton proves this. He adds, that he found the blood liquid and very little coagulable. This is another proof in my favour. I have ſaid, in my former eſſay, that the coagulability of fluids obeys the ſame laws, and depends upon the ſame principle as the irritability of the ſolids; conſequently, the blood deprived of the irritable principle, or of oxygene, ought to be liquid, that is, to poſſeſs little or no coagulability.

The third experiment is a direct proof that

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that the vermilion colour of the blood is owing to the abſorption of oxygene.

Having ſhewn that the arguments upon which the generally received theory of reſpiration is founded are not concluſive, I proceed to ſtate ſome direct experiments in favour of the new theory which I wiſh to eſtabliſh.

A. Experiments upon venous blood.

1. Six ounces of black venous blood, taken from the jugular vein of a ſheep, were introduced into a veſſel filled with oxygene air, and in an inſtant the blood aſſumed a vermilion colour; the thermometer within the veſſel roſe ſeveral degrees, but ſunk again immediately. The mercury in which the veſſel was placed roſe from ſix to eight lines. When the experiment was finiſhed, the blood was increaſed a little in weight; but though I am certain of this increaſe of weight from repeated experiments, I cannot exactly

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exactly aſcertain how much it was, becauſe the inſtruments I made uſe of for this purpoſe were not ſufficiently exact for ſo delicate an experiment. The oxygene air which the veſſel contained was mixed with carbonic acid air, which lime water abſorbed. Some drops of water were formed at the bottom of the veſſel.

This experiment proves, that during reſpiration the blood abſorbs the oxygene; and I make no doubt but it is poſſible to determine the weight of the oxygene abſorbed, by repeating this experiment with inſtruments as exact as thoſe of M. Lavoiſier.

This experiment alſo proves, that during reſpiration there is formed carbonic acid air and water, that is to ſay, that there is an exhalation of the hydrogene air from the blood *.

2. The jugular vein of a ſheep was opened, and the blood which flowed from

* Rather of the baſe of hydrogene air.

it

it was received in a glaſs bottle filled with oxygene air. The bottle, when half full, was cloſed. The blood which it contained immediately aſſumed a vermilion colour, became very fluid, and coagulated but ſlowly into a reddiſh and thick maſs, without any ſeparation of ſerum. On the morrow the bottle was opened in order to examine the air which it contained, and the oxygene air was mixed with carbonic acid air. Some drops of water were formed near the mouth of the bottle.

This experiment confirms the firſt.

3. A conſiderable quantity of very pure oxygene air was injected into the jugular vein of a dog. The animal raiſed moſt terrible outcries, breathed very quickly, and with the utmoſt difficulty; by little and little his limbs became hard and ſtiff, he fell aſleep, and died in leſs than three minutes. Upon opening the thorax and the pericardium, the heart was found more irritable than ordinary, and its alternate contractions and dilatations

latations continued upwards of an hour. The right auricle of the heart was vermilion, and it contained, as well as the right ventricle, a great quantity of blood of a bright vermilion colour, frothy and not coagulated. The blood contained in the left ventricle, in the aorta, and the arteries, was of a roſe colour, and was mixed with bubbles of air. All the muſcles were more irritable than ordinary. After the blood contained in the heart and veins was diſcharged, the irritability of the heart and the muſcles ſenſibly diminiſhed.

This experiment appears to me to prove moſt deciſively, that the vermilion colour which the blood aſſumes in paſſing through the lungs is not owing to the loſs of the carbonic-hydrogene air, but that it proceeds from the combination of the blood with the oxygene air. In the experiment I have deſcribed, the livid colour of the venous blood in the right auricle and right ventricle of the heart was changed to vermilion. Nevertheleſs it could not have loſt any

any carbonic-hydrogene air; it only acquired oxygene. Beſides, this experiment is a direct proof that oxygene is the principle of irritability; for by ſurcharging the blood with oxygene, by hyper-oxygenating it, if I may uſe the expreſſion, the irritability of the blood was, as we have ſeen, conſiderably increaſed.

4. A ſmall quantity of azotic air, which had been expoſed for ſome time to the contact of lime water, in order to ſeparate any carbonic acid air it might contain, was injected into the jugular vein of a dog. The animal died in twenty ſeconds. Upon opening the thorax, the pericardium, and the heart, the right auricle and ventricle were filled with black thick and coagulated blood. The left ventricle was of its ordinary colour. The heart, and almoſt all the muſcles, loſt their irritability almoſt intirely; they contracted but weakly upon the application of the ſtrongeſt ſtimuli, ſuch as ſulphuric æther and the electric ſpark.

5. The

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5. The venous blood of a ſheep was received in a bottle filled with azotic air. The blood coagulated in an inſtant, and aſſumed a colour black as ink. There was a ſeparation of a great quantity of ſerum. The next day, on opening the bottle, a faint ſmell of ammoniac was perceivable. The air was azotic air, which extinguiſhed a light.

In this experiment, the azotic air in contact with the venous blood rendered its colour deeper, and even quite black. The ammoniac produced is owing to the hydrogene air which eſcaped from the venous blood, and united itſelf to the azote.

The colour of the blood becoming deeper after it had loſt part of the hydrogene united with it, ſeems to prove that this deep colour is owing to the carbon of the blood, and not to the combination of hydrogene air, as has been ſuppoſed.

6. A bottle full of carbonic acid air was half

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half filled with the venous blood of a ſheep. It coagulated in an inſtant, aſſumed a very deep colour, and there was the ſeparation of a great quantity of reddiſh ſerum.

7. A ſmall quantity of carbonic acid gas was injected into the jugular vein of a dog. The animal became ſleepy, and died in about a quarter of an hour. The right auricle and ventricle of the heart were filled with thick blood, and in part coagulated. The blood contained in the left ventricle and auricle was of a deeper colour than ordinary. The heart and muſcles had loſt all their irritability.

This experiment proves, moreover, that the deep colour of the venous blood is not owing to the combination of hydrogene air. In this experiment, part of the oxygene of the carbonic acid air probably unites itſelf with the hydrogene of the blood, and forms water, and the carbon, which before was combined with this oxygene, unites with the blood, and gives it its deep colour.

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8. An inciſion was made in the jugular vein of a ſheep, and the blood which came from it was received in a bottle full of nitrous air. When the bottle was half filled, it was cloſed. The blood coagulated immediately, and a ſeparation of a great quantity of blackiſh ſerum took place. The day after, on opening the bottle, a very ſtrong ſmell of nitrous æther (dulcified ſpirit of nitre) was perceived; the nitrous air having been changed in part to nitrous æther by the carbonic-hydrogene air of venous blood.

This experiment proves beyond a doubt that the venous blood contains carbonic-hydrogene air, and that this air is not very intimately united with it, but that it ſeparates with the greateſt eaſe. The nitrous æther produced in this experiment is owing to the union of the carbonic-hydrogene air, which exhales from the blood, with the nitrous air. The blood, after it has loſt this air, does not aſſume a vermilion colour; but, on the contrary, it takes a very deep colour; it is not, therefore, to the union of the

the blood with carbonic-hydrogene, that the deep colour of the venous blood is owing, ſince this colour becomes ſtill deeper when the hydrogene is ſeparated from the blood.

9. A ſmall quantity of nitrous air was injected into the jugular vein of a dog. The animal died in leſs than ſix minutes. The right auricle and ventricle of the heart were filled with blood, thick, black, and partly coagulated. The blood contained in the left ventricle of the heart, was of a much deeper colour than ordinary; the heart had loſt its irritability. The lungs were of a greeniſh caſt, and partly putrefied *. All the canal of the wind pipe was filled with a green foam, that came in great quantities out of the mouth of the animal during the convulſions that preceded its death.

Experiments upon arterial blood.

10. An inciſion was made in the carotid

* The green colour is a ſign of nitrous acid, not of putrefaction. B.

artery of a ſheep, and the blood that iſſued thence was received into a bottle full of oxygene air. The bottle, when half filled, was cloſed. The colour of the blood became in an inſtant of a bright vermilion. The next day the bottle was opened, and the oxygene gas which it contained was found mixed with a very ſmall quantity of carbonic acid gas.

11. The arterial blood of the carotid artery of a ſheep was received into a bottle full of azotic air. The bottle, being half filled, was cloſed; the blood coagulated at the ſame moment, and aſſumed a very deep colour. On opening the bottle the next day, the azotic air which it contained was found mixed with a ſmall quantity of oxygene air, ſo that a candle burnt in it for near two minutes.

This experiment proves deciſively,

1. That arterial blood contains oxygene air.

2. That

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2. That it is to its combination with this air that its vermilion colour is owing; and that it aſſumes its deep colour as ſoon as it is deprived of its oxygene air.

12. Three ounces of vermilion blood, from the carotid artery of a ſheep, were received upon a plate, which was immediately placed under a veſſel filled with carbonic acid air. The blood did not change its colour, but continued the ſame for ſome hours.

13. Arterial blood, from the carotid artery of a ſheep, was received into a bottle filled with carbonic acid air. No change in the vermilion colour.

Theſe two experiments prove that carbonic acid air has no action upon arterial blood, although it has a very great one upon venous blood.

14. The arterial blood of the carotid artery of a ſheep was received into a bottle

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full of nitrous air. The bottle, when half filled, was closed. The blood contained in it coagulated immediately, and assumed a green colour upon the surface. A small quantity of greenish serum was separated. The day after, on opening the bottle, the vapours of nitrous acid were observed by all who were present.

Here then is an experiment which proves, in a most decisive manner, the presence of oxygene in the arterial blood; since it is from this circumstance alone that it is capable of changing nitrous air into nitrous acid. The green colour, observed in this and the 9th experiment, arises from part of the azote separating itself from the nitrous air.

15. Arterial blood, from the carotid artery of a sheep, was received into a bottle full of hydrogene air, which was closed when half filled. The blood became of a brighter vermilion, and remained fluid for some time. It coagulated at last, and a small quantity of serum

ſerum was ſeparated. The day after, the hydrogene air contained in the bottle was found mixed with a ſmall quantity of oxygene air, which the nitrous air abſorbed. This experiment proves the preſence of oxygene air in arterial blood.

16. The arterial blood, as before, was received in abottle containing equal portions of oxygene and hydrogene air. The bottle, when half filled, was cloſed. The blood in the bottle became moderately hot, remained fluid, and was of a more vermilion colour. It coagulated at laſt, and a ſmall quantity of ſerum was ſeparated. The day after, the air in the bottle was mixed with a ſmall quantity of carbonic acid air, of which the preſence was aſcertained by lime water.

17. A ſmall glaſs tube was filled with arterial blood of a bright vermilion; it was ſealed hermetically, and expoſed to the light. The blood changed its colour

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by degrees, and in ſix days became as black as venous blood.

18. The ſame experiment was repeated, with this difference only, that the tube was expoſed to heat, and not to the light. The blood became black in a ſhorter time.

The 17th and 18th experiments made by Dr. Prieſtly, and repeated afterwards, appear to me to demonſtrate, that it is not to the contact with hydrogene air that the venous blood is of a black colour.

I conclude from theſe experiments,

1. That the change of colour the blood undergoes during circulation is not owing to its combination with hydrogene air.

2. That the deep colour of the venous blood, is owing to the carbon it contains.

3. That the vermilion colour of the arterial

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terial blood proceeds from the oxygene with which the blood is combined, during its paſſage through the lungs.

4. That reſpiration is a proceſs exactly analogous to the combuſtion and oxidation of metals; that theſe phænomena are the ſame, and to be explained in the ſame manner.

5. That during circulation, the blood loſes its oxygene, and charges itſelf with carbonic-hydrogene air, by means of a double affinity.

6. That during the diſtribution of the oxygene through the ſyſtem, the heat which was united with this oxygene eſcapes; hence the animal heat.

7. That the great capacity of the arterial blood for heat is owing to the oxygene with which it is united in the lungs.

Having ſhewn that the blood is oxygenated in its paſſage through the lungs; that in

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in the circulation it loſes the oxygene it had abſorbed; and that it returns to the lungs ſurcharged with carbonic-hydrogene air, it remains to prove, that to this oxygene, diſtributed through every part of the ſyſtem, is owing the irritability and the life of organized bodies. Here follow the proofs upon which this theory is founded.

The irritability of organized bodies is always in a direct ratio of the quantity of oxygene they contain.

1. Every thing that increaſes the quantity of oxygene in organized bodies, increaſes at the ſame time their irritability.

We have ſeen a direct proof of this in the 3d experiment, cited above. Beſides this, a great number of other phænomena ſupport my opinion. The irritability of animals made to breathe oxygene air is wonderfully encreaſed. Blanched plants, whoſe irritability has been accumulated in

conſequence

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conſequence of the abſtraction of the ſtimulus of light, contain a great quantity of oxygene, according to the experiments of Mr. Fourcroy. I have obſerved, in the courſe of my experiments, that plants made to grow in oxygene air become white, although expoſed to the light. But what ſhows more clearly than all, that the irritability is always in proportion to the quantity of oxygene, are the phænomena attending the action of mercury and mercurial ſalts upon animals. As this is one of the moſt ſtriking proofs of my theory, and as I have before obſerved, that many perſons, and amongſt the reſt philoſophers of the firſt rank, ſuch as Dr. Crawford, have been ſtruck with the novelty and ſimplicity of my mode of explaining theſe phænomena, I cannot forbear entering into ſome detail upon this ſubject. It is a well-known fact amongſt phyſicians, that mercury, in its metallic ſtate, has no effect upon the human body. I have known many people, who for many years took a daily portion of quickſilver, to the amount of one or two ounces, from an

idea

idea of guarding themſelves from epidemic diſeaſes, but who never perceived any effect whatever from this ſingular cuſtom. It is proved alſo by the experiments of Dr. Saunders, that the effects of mercurial ointment are owing only to the ſmall quantity of mercury that has been oxidated during a long trituration. It is neceſſary, therefore, that mercury ſhould be oxidated, to have any effect upon the human body. On the other hand it is well known, that in perſons who have rubbed themſelves with mercurial ointment, or who have taken the oxid of mercury, the mercury, after having produced its uſual effects, has paſſed through the ſkin in a metallic form, and has amalgamated itſelf with watches, and the gold in the pocket, &c. The oxid of mercury, in paſſing through the human body, parts with its oxygene, and it is to this oxygene alone, which remains combined with the ſyſtem, that the effect produced by oxidated mercury is owing. This effect is the mercurial diſeaſe, the ſymptoms of which are the ſame as thoſe of the ſcurvy; the mouth,

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mouth, gums, and the whole ſyſtem are affected in a manner extremely analogous. But the ſcurvy, as I have proved in my firſt eſſay, is a diſeaſe produced by the accumulation of the irritable principle *. The accumulation, therefore, of the oxygene producing the ſame effects, the great analogy between the irritable principle and oxygene appears to be proved, and I think myſelf authorized to conclude, that oxygene is the principle of irritability.

(M. Berthollet, in the Paris Memoires, 1780, has attributed the cauſticity of metallic oxids to the oxygene they contain).

2. Whatever diminiſhes the quantity of oxygene in organized bodies, diminiſhes at the ſame time their irritability.

This has been ſhewn in the 9th experiment, where the heart and the muſcles loſt their irritability, having been deprived of their oxygene by nitrous air. But not to

* Certainly erroneous.

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leave any doubt upon this ſubject, I made the following experiment.

Experiment 19. The heart of an animal juſt killed was cut into pieces, and put into a glaſs retort, to which was affixed a pneumatic apparatus. A very ſmall degree of heat was applied to it, by means of a lamp placed under the retort. When the pieces were heated, bubbles of air were perceived in the pneumatic apparatus. They remained expoſed to the ſame degree of heat for nearly two hours, till the ſurface was juſt burnt. Upon examining the air which had paſſed into the apparatus, it was found that the firſt portion of air was the atmoſpheric air of the retort, mixed with a very ſmall quantity of vital air, whoſe preſence was aſcertained by nitrous air. The ſecond was vital air mixed with carbonic acid air.

I have repeated this experiment upon many other parts of animals juſt killed, and I have always obtained a greater or a leſs quantity of oxygene air. It is poſſible to ob-

tain the ſame quantity of this air many times following, by expoſing the animal ſubſtances alternately to the atmoſpheric air, and to a heat of 60 or 70 degrees of Reaumur. I ſhall obſerve, however, that theſe experiments are very difficult to make, and ſome time is requiſite to aſcertain the degree of heat neceſſary to diſengage the oxygene air. If the heat applied be too great, carbonic acid air will come over inſtead of oxygene air. It is poſſible to extract all the oxygene which animal ſubſtances contain, by means of hot water: it is thus we make jellies. Theſe jellies are always more or leſs tranſparent, which, without any other proof, would be ſufficient to authorize us to ſuppoſe the preſence of oxygene in jellies, becauſe it is certain that all tranſparent bodies except alcohol and æther owe their tranſparency to the oxygene that enters into their compoſition *. I have proved, that oxygene combines with venous blood in the lungs; that it is diſtri-

* This is reaſoning very rapidly. Who has detected oxygene in rock cryſtal?

buted

buted to all parts of the ſyſtem by the circulation; that to this principle irritability is owing: it remains only to examine what becomes of the great quantity of oxygene which all parts of the ſyſtem are continually receiving from the blood. I ſhall attempt to prove, that the different ſtimulating ſubſtances abſorb this oxygene.

I have obſerved in my firſt eſſay, that there are three different ſtates of the organized fibre.

1. The ſtate of health or tone of the fibre.

2. The ſtate of accumulation, in which the fibre is ſurcharged with the irritable principle.

3. The ſtate of exhauſtion, in which the fibre fails through want of the irritable principle.

All ſubſtances capable of coming in contact

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tact with the irritable fibre, can likewiſe be ranged under three claſſes, of which

The firſt comprehends the ſubſtances which have the ſame degree of affinity to the irritable principle, or the oxygene, as the organized fibre itſelf. Theſe ſubſtances produce no effect upon the fibre.

The ſecond contains thoſe which have a leſs degree of affinity to the oxygene than the fibre has. Theſe ſubſtances, coming in contact with the fibre, will ſurcharge it with oxygene, and produce the ſtate of accumulation. Theſe ſubſtances may be called negative ſtimuli.

The third claſs contains thoſe ſubſtances which have a greater degree of affinity to the oxygene than the fibre itſelf has. Theſe, coming in contact with the fibre, will deprive it of its oxygene, and produce the ſtate of exhauſtion. I ſhall call theſe ſubſtances poſitive ſtimuli.

It is a fact known at this time, that the

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affinity of different ſubſtances varies conſiderably according to the degree of temperature. The ſame variety takes place in the organized fibre. I ſhall obſerve, therefore, in order to be exact, that when I ſpeak gerally of the affinities of the irritable fibre, I mean always in the ordinary temperature of the blood of warm animals.

I will make ſome obſervations upon each of theſe claſſes.

The firſt claſs comprehends, as I have ſaid, ſubſtances having the ſame degree of affinity to the oxygene as the irritable fibre. All organized, or living ſubſtances, are to be ranked under this claſs. (Note, The words organized and living are, in my opinion, ſynonimous. I regard as living, every body, each part of any body, in a word, all organized ſubſtances, as long as they contain the principle of irritability, or of life, and as long as the affinities are the ſame as thoſe of living ſubſtances. The wood, for inſtance, of which our chairs and tables are

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made, is an organized or living ſubſtance; and to ſpeak properly, it cannot be ſaid that the wood is dead before it be rotten, and ſo of the reſt. Our ideas of life and death are very vague ideas, and I ſhall attempt to fix them in ſome other way). Theſe ſubſtances produce no effect upon the irritable fibre, while their degree of temperature is the ſame as that of the fibre with which they come in contact.

I have arranged, in the third claſs, the poſitive ſtimuli, that is to ſay, thoſe ſubſtances which have a greater degree of affinity to the oxygene than the fibre has. Theſe ſubſtances, coming in contact with the fibre, combine with the oxygene it contains, deprive it of its irritability, and leave it in a ſtate of exhauſtion. There is a great number of theſe ſubſtances. The moſt known are alcohol, ſulfuric æther, opium, and other narcotics, oil of lauroceraſus, and oils in general, greaſe, ſugar. All theſe ſubſtances are combuſtible, that is to ſay, they have a great affinity to oxy-

gene, and it is by this property that they deprive the organized fibre of its irritability, by combining with the oxygene it contains. The ſecond claſs comprehends the negative ſtimuli, or ſubſtances, which have a leſs affinity to the oxygene than the fibre has. Some of the moſt terrible poiſons we know of muſt be ranked under this claſs. The oxygene which combines with the organized fibre, when it comes in contact with theſe poiſons, renders it ſo extremely irritable, that the weakeſt ſtimulus is capable of producing death; by a law of irritability which has been explained in the firſt eſſay. Oxygenated marine acid is for this reaſon ſo fatal a poiſon to all organized bodies. It deſtroys them by ſurcharging them with irritability, that is, by hyper-oxygenating them, and becomes marine acid by this operation.

Arſenic, under its metallic form, has no effect upon animals; but the white oxid of this metal is one of the moſt terrible poiſons; for it hyper-oxygenates the organized fibre

fibre with which it comes in contact, and re-aſſumes its metallic form. Oxygenated metallic ſalts, ſuch as the corroſive or muriatic ſublimate of mercury oxygenated, &c. produce the ſame effects. The oxids of ſilver and mercury produce greater or leſs effects upon the organized fibre, in proportion as they contain more or leſs oxygene. The black oxid of mercury, otherwiſe called æthiops, produces the moſt trifling effects; the red oxid of the ſame metal produces the moſt terrible effects, and deſtroys organized bodies in a very ſhort time. The ſame explanation applies to the action of ſulfate of tin and lead, and the acetate of lead and braſs upon the organized fibre.

I am convinced by experiments which I ſhall relate ſome other time, that the organized fibre, both animal and vegetable, decompoſes the water that comes in contact with it. The greateſt part of the water we drink is firſt decompoſed, and then recompoſed. It is indeed one of the means

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by which nature furniſhes organized bodies with the oxygene neceſſary to preſerve their irritability and life. This diſcovery explains very many phænomena hitherto inexplicable. I have alſo reaſon to think, that by this diſcovery of the decompoſition of water by organized bodies, we ſhall be able to explain the moſt hidden myſteries of animal phyſiology. Reflecting upon the reſults of ſeveral of my experiments, I begin to ſuppoſe that the hydrogene air, which remains after the oxygene of the water is united to the irritable fibre, may ſerve to ſupply the loſs of nervous fluid, or, in other words, I ſuppoſe that the nervous fluid is the hydrogene air, perhaps carbonic-hydrogene gas. I confeſs this is only a conjecture, which I am not yet able to prove, but which appears to me very probable. Be this however as it may, it is very certain that water is decompoſed and recompoſed continually in organized bodies. This is clear from experiments I ſhall hereafter enumerate.

I have explained the phænomena of hunger in animals; I have ſaid, that this ſenſation

ſenſation was the conſequence of irritability accumulated in the ſyſtem; and that for a ſubſtance to be nutritious, it muſt be a poſitive ſtimulus; namely, one that has a great tendency to unite itſelf to the oxygene, becauſe it is only by uniting itſelf with this principle, with which the ſyſtem is ſurcharged, that it can reſtore the tone of the fibre, and allay the painful ſenſation of hunger. Every phænomenon ſupports this theory. Different ſubſtances nouriſh only in proportion to their affinity to oxygene. Living animal ſubſtances (oyſters for inſtance) afford little or no nouriſhment, becauſe they cannot combine with the oxygene, with which they are already ſaturated; hence the common obſervation, that oyſters increaſe the appetite. Animal jellies, fruits, vegetable ſubſtances in general, afford little or no nouriſhment. Animal food juſt killed does not nouriſh ſo much as that which has been kept ſome time; and raw meat is not ſo nouriſhing as that which has been cooked. Hence all the art of cookery, which conſiſts only in depriving

the food of its oxygene, by applying different ſtimulating ſubſtances, and, above all, the ſtimulus of heat. Roaſting the food is the moſt ſimple manner of cooking it; whilſt it is expoſed to the heat, it parts with its oxygene, as in the 19th experiment. Oils, fat, ſugar, alcohol, and other ſubſtances, which have a great affinity to oxygene, are very nouriſhing. In the Eaſt Indies, millions of men ſupport themſelves by ſmall quantities of opium, when the rice harveſt fails them, as very frequently happens in thoſe wretched countries, groaning under the deſpotiſm of a company of Engliſh merchants.

Thirſt is a ſtate of the ſyſtem oppoſite to that of hunger; it is a ſenſation which indicates a ſtate of exhauſtion, a deficiency of oxygene. Every thing that reſtores to the fibre its loſt oxygene, puts an end to this diſagreeable ſenſation. Water produces this effect by its decompoſition when it comes in contact with the fibre. The ſame effect will be produced by vegetable acids, which

which are always decompoſed in the ſtomach of animals. It is only in proportion to the oxygene in the compoſition of the acids, and to which they have but little affinity, that they refreſh and allay the ſenſation of thirſt. Thus vegetable acids are the beſt remedies againſt the effects of narcotic poiſons; for by their decompoſition they reſtore to the fibre the oxygene which the poiſons had deprived it of. Vinegar, taken in large doſes, cures the ſtate of exhauſtion produced by a ſtrong doſe of opium, and prevents death, which would otherwiſe enſue. It is well known that drunken perſons become ſober by drinking a glaſs of vinegar; that is, the vinegar reſtores the tone of the ſyſtem which it had loſt, by the effect of the alcohol contained in the wine. A great quantity of water produces the ſame effect.

Many other phænomena may be explained upon the ſame principle. We find the air freſher and more agreeable after heavy rain, becauſe the watery vapours which

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which riſe from the earth, and come in contact with our bodies, are decompoſed and reſtore the loſt oxygene *. The phænomena diſplayed by the rotifer, that ſingular inſect, which, though entirely dried up, may be revived by moiſtening it with a drop of water, appear inexplicable; but it ſeems that it is eaſy to account for it on my principles. The drop of water is decompoſed, the oxygene it contains combines with the rotifer, reſtores its irritability, its life, and organic motion, of which it had been deprived by the ſtimulus of heat, to which it had been expoſed in becoming dry.

Amongſt the known poſitive ſtimuli, thoſe which produce the greateſt effects are the ſtimulus of putrid fevers, or of the plague, and that of the mephitis, which exhales from putrefied animal ſubſtances in places where the air cannot enter, as in tombs and burial places. The affinity which this mephitic gaz has for oxygene is ſo great,

* Nonſenſe. The air is only become a better conductor of heat.

that

that as ſoon as it comes in contact with the fibre, it deprives it of its oxygene, and cauſes death, frequently in an inſtant. The beſt way to prevent the fatal effects of this gaz is, to detonate nitre upon burning charcoal. The oxygene gaz which eſcapes during the decompoſition of the nitre ſupplies the oxygene which combines with the mephitic air. This theory is ſo true, that the workmen who have been ſuffocated by the mephitic air exhaling from tombs have recovered their ſenſes and been refreſhed (according to their own expreſſion) as ſoon as they have been made to reſpire oxygene air.

I ſhall hereafter relate the experiments I have made upon vegetables, with many ſtimulating ſubſtances, but, above all, with alcohol, opium, the ſolution of white oxid of arſenic, vinegar, water, heat, and the oxids of mercury. I have found that theſe ſubſtances had effects upon plants ſimilar to what they had upon animals; that the irritability of the moſt irritable plants, ſuch as

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as the mimoſa and hediſarum, may be entirely deſtroyed by poſitive ſtimuli, by opium for example, alcohol, or heat; and that it is poſſible to give very ſenſible irritability to plants which did not appear to poſſeſs it, by applying, for ſome time, negative ſtimuli, ſuch as vinegar, or white oxid of arſenic. I hope that the reſult of theſe experiments will be of conſiderable uſe to agriculture, by ſhewing us the true nature of plants, their diſeaſes, and the means of remedying them. I have found that oils and alcohol, employed in ſmall quantities, are ſpecific remedies for the diſeaſes of plants, produced by the accumulation of the irritable principle; diſeaſes marked by the yellow colour which the leaves aſſume.

IT is eaſy to ſupply the latter part of this theory with a neceſſary correction. It is not credible that many poſitive ſtimuli, ſuch, for inſtance, as the vegetable poiſons of the tropical countries, and the venom of certain ſerpents, which produce death, in quantities ſo aſtoniſhingly minute, ſhould, as Dr. Girtanner

ner imagines, exhauſt the irritable principle, by combining with it directly themſelves; neither, ſurely, can alcohol, opium, and oil of lauro-ceraſus, be ſuppoſed to attract from the irritable fibre, if we conſider only the quantity in which they produce their effects, a large quantity of oxygene. Is it not more likely that they occaſion, throughout the whole ſyſtem, a new combination of oxygene? The blood, and the muſcles of animals deſtroyed by poſitive ſtimuli, and of thoſe deſtroyed by negative ſtimuli, as nitre or arſenic, ought, according to this idea, to exhibit appearances diametrically oppoſite.

It is alſo evident, that Dr. Girtanner's, or, more properly ſpeaking, Dr. Brown's, Materia Medica is too ſcanty: negative ſtimuli will be neceſſary to correct certain aberrations of the ſyſtem from health. His idea of negative ſtimuli, as I often obſerved to Brown and his diſciples in 1785 and 6, ſupplies one of the greateſt defects of Brown's ſyſtem. It is in vain to ſay of thoſe ſubſtances

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ces which directly, and without any appearance of previous excitement, in whatever quantity they are adminiſtered, ſuch as lead, diminiſh the actions of life, that they are leſs powerful ſtimuli; for how, on this principle, can they leſſen the effect of the ordinary ſtimuli, which are applied at the ſame time? It is equally in vain to ſay (what Dr. Girtanner repeats), that the depreſſing paſſions are only the abſtraction of the ſtimuli of the exciting paſſions. Univerſal experience, I apprehend, will immediately reject ſuch a ſcale of mental affections, as this ſyſtem ſuppoſes; a ſcale which muſt preſent the paſſions ſomewhat in this order; ſuppoſing the moſt exciting to ſtand uppermoſt:

ANGER,
JOY,
HOPE,
CONTENT,
INDIFFERENCE, } As far as the mind is capable of indifference.
FEAR,
SORROW, and ſo on.

Now

Now is it credible or poſſible, that ſorrow ſhould be to indifference what darkneſs is to twilight? Every individual almoſt is capable of judging, whether the thoughts of a perſon in affliction are leſs buſy than of an idiot? whether the mind of one under the influence of the diſtreſſing paſſions is leſs upon the ſtretch, than of him, "Who "whiſtles as he walks for want of thought?"

During ſuch a ſtate of vacancy, it will hardly, I preſume, be thought that there is more mental exertion than in the moſt profound ſorrow, even if we admit the common ſuppoſition, that in this ſtate the mind reſts upon the contemplation of one or a few ideas; a ſuppoſition which requires much limitation.

Brown very properly warns his readers of the methaphorical nature of his terms.* We

* *Partim ob incertam rei naturam, partim ob ſermonis egeſtatem, item hujus doctrinæ novitatem, incitabilitas modo* abundare, *cum ſtimuli parum admotum, modo* deficere, exhauriri *aut* conſumi, *cum is vehementius incubuit, paſſim*

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We muſt therefore, without regard to them, if obſervation indicates it, admit a power in ſome drugs, in ſome of the paſſions, and

paſſim deinceps dicetur. Tum hic, cum alias, ubique REBUS VERIS STANDUM: *lubrica cauſarum utpote ferè incomprehenſibilium, quæſtio, venenatus ille philoſophiæ anguis, cum curâ fugienda. Nequis igitur, per modo relata dicta, incitabilitatis naturam reſpici, aut, an materia ſit, et ſic modo augeatur, modo imminuatur: an adhærens materiæ facultas, nunc vigeat, nunc langueat, definiri: aut ullo modo reconditam quæſtionem attingi, quod magno ſcientiæ malo ſemper ferè factum, interpretetur.* l. c. I. 5. 6.

The warning againſt the enquiry concerning cauſes, as being incomprehenſible, requires ſome explanation. Cauſes are only phænomena, uniformly preceding other phænomena; they are, therefore, juſt as comprehenſible as effects: we ceaſe to comprehend juſt where we ceaſe to perceive. The bane of philoſophy is the transferring of phænomena, by the imagination, from one part of nature to another, where the ſenſes diſcern no traces of their exiſtence; and placing them as antecedents before obſerved phænomena. As this imaginary ſucceſſion will be wrong 999 times at leaſt in 1000, we ſhall as often fail when we attempt, in practice, to modify the conſequents, by regulating the ſuppoſed antecedents.—But, by diſcovering the real ſucceſſion of phænomena, we ſhall aſſuredly gain infinite advantages. Thus, were oxygene the principle of irritability, we ſhould not only

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and in ſome external circumſtances, either of preventing the ſyſtem from giving out its excitability, or from accumulating it (which ſtate is very often ſeen, where a continuance of ſleep, ſufficient as to its duration, does not refreſh, or, according to Cullen's expreſſion, *render the ſyſtem more liable to be affected by ſtimuli of all kinds)*, or on the contrary, of accumulating it too rapidly. We are even enabled, by the foregoing hypotheſis, to conceive a modus operandi in all theſe caſes.

Theſe principles, with which the late wonderful diſcoveries of Mr. Galvani, Valli, and Volta, ſeem perfectly well diſpoſed to coaleſce, promiſe all thoſe advantages which would reſult from a perfect knowledge of the mechaniſm of the animal functions. Was

only have the ſatisfaction of comprehending more clearly the actions of the living ſyſtem, but we ſhould have them more in our power, than while we were ignorant of this truth. While, therefore, we religiouſly ABIDE BY FACTS, we can do nothing ſo well as inveſtigate cauſes.

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not Mayow, for inſtance, infinitely nearer the truth, than any author of a later hypotheſis, when he imputed muſcular motion to the *effervescence of his nitro-atmoſpherical particles?* Does not muſcular contraction or intumeſcence really depend upon the combination of oxygene with hydrogene and azote (ſeparately and combined, in various proportions), in conſequence of a ſort of exploſion produced by the nervous electricity? According to this hypotheſis, animal motion, at leaſt that of animals analogous to man, would be produced by a very beautiful pneumatic machinery; and our nervous and muſcular ſyſtems may be conſidered as a ſort of ſteam-engine: This hypotheſis, though not perhaps at this moment capable of ſtrict proof, is extremely probable, ſince it is countenanced by every obſervation and experiment yet made on the ſubject. It accounts for the perpetual neceſſity of inhaling oxygene, and enables us to trace the changes undergone by this ſubſtance, from the moment it is received, till the moment it is expelled. By the blood it is imparted

to the muſcular fibres; here, during their contraction, it combines with the elements above-mentioned into water and various ſalts, among which the marine and phoſphoric acids deſerve particular notice. In this ſtate it is taken up by the abſorbents, and afterwards exhaled, or excreted. Obſervations, adduced in the preceding pages, ſufficiently ſhew the neceſſity of oxygene to muſcular motion; and how the power of motion languiſhes, when this principle is ſcantily ſupplied. It appears that *meat* becomes tender in conſequence of the ſecondary combination of oxygene, in whatever way this ſecondary combination be effected; whether by keeping it till the putrefactive proceſs takes place more or leſs; by cookery; by obliging the animals to undergo violent exerciſe before death, as in hare-hunting, bull-baiting, and in an expedient of gluttony, rather more barbarous than either of the preceding, that of flogging poultry to death. The fleſh of animals ſo deſtroyed ought to be more ſucculent, as well as more tender. It is an obſervation

of experienced ſportſmen, that an hunted hare will continue to emit ſteam, when brought to table, very much longer than an hare otherwiſe killed. I have heard the ſame remark made with reſpect to hunted veniſon. Theſe phænomena correſpond perfectly with the ſuppoſition of liquids, partly volatile, being generated during muſcular action.

In the Weſt India Iſlands they kill their poultry with vegetable poiſon, in order to render them tender without keeping. Stimuli, which are only leſs violent poiſons, are ſometimes uſed for the ſame purpoſe in this country. It does not appear whether they produce their effect immediately, or by firſt exciting the nervous electricity. But whatever be the mode of action of theſe poiſons and ſtimuli, that of contagious miaſmata ſeems to be exactly the ſame. *

The

* I once ſaw an inſtance, in which I could not doubt that complete intoxication was produced by the contagion of typhus, to which the perſon had been much expoſed. One morning, immediately upon riſing, and I knew that he had been perfectly ſober the night before,

The ſimilarity of the ſymptoms in typhus and ſcurvy has been frequently noticed; and the ſimilar ſymptoms of theſe diſeaſes ſeem evidently to depend upon the ſame cauſe; the contagion of typhus depriving the ſyſtem of oxygene, by cauſing the combination of a great part of that which it already contains. Hence it is probable that the true indication of cure in typhus is to reſtore the oxygene; and it is likely that

fore, I was aſtoniſhed to obſerve that ſlighty vivacity and diſpoſition to wild disjointed talk, together with the other ſigns that infallibly denote a certain degree of intoxication, eſpecially when you are well acquainted before-hand with the manners of the party. In the courſe of the day, during which I ſaw him frequently, he became heavy, had febrile ſhiverings, and complained of head-ache. The next day he became more feveriſh, but was not confined till the fifth day, though the head-ache and other ſymptoms never quitted him. He paſſed through all the ſtages of typhus, but never ſeemed to be in imminent danger. In moſt inſtances, the period of the excitement of the brain is not perceived; we, however, frequently ſee the action of the vaſcular ſyſtem increaſed at the onſet of typhus; this increaſe of action ſometimes miſleads practitioners into the fatal meaſure of blood-letting.—Does not the highly ſaline urine in febrile diſeaſes, and after exerciſe, depend on the chemical combinations above-mentioned?

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upon this principle, a certain and ſpeedy cure will be contrived. The modern practice, which employs ſtimulants very freely, though, upon the whole, I believe, not ſo miſchievous as the contrary, is not ſuch as we ought by any means to acquieſce in. It does not enſure ſo much ſucceſs as might be expected from a method founded on juſt principles; and, indeed, as far as I can learn, the different methods in uſe anſwer pretty much alike, and the diſeaſe is very little in the doctor's power—Oxygene may be more beneficial at one ſtage of typhus than at another.

Thoſe caſes where typhus attacks perſons after expoſure to ſevere cold, at a time when you cannot, by the ſtricteſt ſcrutiny, diſcover any previous veſtiges of contagion in the neighbourhood, render it highly probable that this diſeaſe may be produced by ordinary ſtimuli applied to excitability much accumulated. The ſymptoms of the influenza, which are ſcarce diſtinguiſhable from thoſe of catarrh, as well as the effect produced by the ſteward's viſit to the natives of St. Kilda, (if this reſpectably atteſted,

teſted, though ſurpriſing, narrative be true) afford another inſtance where diſeaſes, extremely ſimilar, at leaſt, are produced by ordinary ſtimuli, and by the extraordinary ſtimulus of contagion. If the marſh-miaſma be not an imaginary being, there is reaſon to preſume the ſame thing of intermittents, which very often appear where marſh-miaſma cannot well be ſuppoſed preſent.

One may, I conceive, reaſonably expect to remove the feveriſhneſs, or indirect debility, that follows intoxication, by cauſing the perſon who is ſuffering under it to reſpire oxygene air. This would, perhaps, not only make up the waſte of this principle, but alſo reſtore the nervous electricity; a circumſtance to which it will always be neceſſary to attend in diſorders of excitement, or produced by excitement. From the experiments of Mr. Sauſſure and Volta, on the electrical phænomena attending condenſation, it may be conjectured that the animal electricity is renewed by reſpiration.

The want of ſome certain method of effecting this will, perhaps, produce ſome doubt in the reader's mind reſpecting the certain efficacy of an hyper-oxygenated atmoſphere in typhus; but the few imperfect trials that have hitherto been made upon the reſpiration of oxygene air, ſeem to me to more than counterbalance this doubt. The experiments of Mr. Fourcroy, the beſt upon record, ſeem to promiſe the happieſt ſucceſs.

Attention is undoubtedly not leſs due to the other elements of organized bodies; and if the importance of oxygene ſeems to have been magnified in the foregoing obſervations, it is only becauſe we have few or no facts which afford a foundation for reaſoning concerning the connection of an exceſs or deficiency of hydrogene or azote with the functions of life: and yet much obſcurity and many difficulties muſt be expected to remain, till we acquire the knowledge of ſuch facts. This reflection ſhould render us only the more attentive to the phæno-

mena

mena of life; for if we can but perceive enough to ſuggeſt a new hypotheſis, capable of being verified by experiments, phyſiology will not fail to gain ſomething, and perhaps ſomething conſiderable, even by the proof of its falſehood. This reflection ſhould alſo teach us to ſet a due value on our preſent knowledge, though it be imperfect; and it ſhould reſtrain thoſe rude hands that are ever ready to pluck up the tender plants of ſcience, becauſe they do not bear ripe fruit at a ſeaſon when they can only be putting forth their bloſſoms.

A boundleſs region of diſcovery ſeems to be opening before us. Phyſical ſcience, which began with remote objects, now promiſes to unfold to us the more difficult and more intereſting knowledge of ourſelves. This kind of knowledge will aſſuredly, as Dr. Girtanner obſerves, become a part, and the moſt important part, of education; and it will effect a greater improvement in the morals of mankind, than all the ſermons that ever have been, or ever will be

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be preached. Phyſiological ignorance is the moſt abundant ſource of our ſufferings; every perſon accuſtomed to the ſick, muſt have heard them deplore their ignorance of the neceſſary conſequences of thoſe practices, by which their health has been deſtroyed; and when men ſhall be deeply convinced, that the eternal laws of nature have connected pain and decrepitude with one mode of life, and health and vigour with another, they will avoid the former, and adhere to the latter. And as actions are named immoral from their effects, ſelf-love and morality are ſo far perfectly the ſame. Nor is this ſort of morality likely to terminate in itſelf; but the habit of acting with conſideration, and upon principle, will extend from the ſelfiſh to the ſocial actions, and regulate the whole of life.

If it be ſaid, that the general rules of health have long been deduced from univerſal experience, it may be anſwered, that the difference between the perception of two events, which, though ſeemingly allied, are

placed

placed at a great diſtance from each other, with a blank between them, and a diſtinct view of the whole ſucceſſion of operations that inſeparably connect them, is immenſe. In the former caſe, there will ſeem to be room for the caprice of chance to intervene and ſeparate them; in the ſecond, there cannot be room for any ſuch deluſive expectation; and this more particular information will render it much more difficult for a perſon to impoſe upon his own underſtanding, than it has hitherto been.

It has always, indeed, appeared to me an eſſential requiſite in a tolerably conſtituted ſeminary of knowledge, to provide the means of popular information on the means of preſerving health; but peculiar advantage may now be expected from ſuch an inſtitution. Its extreme imperfection has hitherto rendered medicine a particular craft, little worth ſtudying, but for the money it would bring. A medley of error, nonſenſe, and contradiction, was not likely to engage

engage volunteer ſtudents; for nothing is more intolerable to moſt minds, than to lie toſſing upon a ſea of doubt. At preſent there is beginning to appear, in phyſiology and pathology, ſomething like the ſimplicity and certainty of truth. In proportion as the laws of animal nature come to be aſcertained, the ſtudy will be gradually eſteemed more worthy of general attention, and in ſpite of the diſguſt raiſed by anatomy at firſt, it will finally prove the moſt popular, as being the moſt curious and intereſting, branch of philoſophy; and a *New Medicine* will at length ariſe, with healing on her wings, from the aſhes of the *Old*.

ADDENDA.

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ADDENDA.

THE following is the paſſage of the *Flora Lapponica* alluded to at page 58. It were to be wiſhed that the few pages of incidental obſervations, occurring in that work, were printed ſeparately. In ſpite of much quaintneſs of language and puerility of ſentiment, they would intereſt many readers, not likely to meet with them in their preſent ſituation :—

O felix Lappo! qui in ultimo angulo mundi ſic bene lates contentus et innocens. Tu nec times annonæ charitatem, nec Martis prælia, quæ ad oras tuas pervenire nequeunt, ſed florentiſſimas Europæ provincias et urbes, unico momento, ſæpe dejiciunt, delent. Tu dormis hic ſub tuâ pelle ab omnibus curis, contentionibus, rixis liber, ignorans quid ſit invidia. Tu nulla noſti niſi tonantis Jovis fulmina. Tu ducis innocentiſſimos tuos annos ultra centenarium numerum cuni facili ſenectute et ſummâ ſanitate. Te latent myriades morborum nobis Europæis communes. Tu vivis in ſylvis, avis inſtar, nec ſementem facis, nec metis, tamen alit te Deus optimus optime. Tua ornamenta ſunt tremula arborum folia, graminoſique luci. Tuus potus aqua cryſtallinæ pelluciditatis, quæ nec cerebrum inſaniâ adficit, nec ſtrumas in Alpibus tuis producit. Cibus tuus eſt vel verno tempore piſcis recens, *vel æſtivo ſerum lactis, vel autumnali tetrao, vel hiemali, caro* recens *rangiferina abſque* ſale et pane, *ſingulâ vice unico conſtans ferculo, edis, dum ſecurus e lecto ſurgis, dumque*

dumque eum petis, nec noſti venena noſtra, quæ latent ſub dulci melli. Te non obruit ſcorbutus, nec febris intermittens, nec obeſitas, nec podagra, fibroſo gaudes corpore et alacri, animoque libero. O ſancta innocentia, eſtne hic tuus thronus inter Faunos in ſummo ſeptentrione, inque viliſſimâ habitâ terrâ? numne ſic præfers ſtragula hæc betulina mollibus ſerico tectis plumis? Sic etiam credidere veteres, nec male.

HAPPENING, in a company where the Reverend Mr. Leſlie was preſent, to mention my opinion of the poſſible good effects of air, containing leſs oxygene than common, in conſumption, he related ſome circumſtances relative to the academy at Liege, which he thought gave ſome countenance to my idea; and afterwards favoured me, at my requeſt, with the following particulars:—

Cum a me, vir eruditiſſime, poſtules ut chartæ ea commendem quæ nuper in familiari colloquio aſſerui circà modum procedendi cum adoleſcentibus in academiâ Anglorum Leodii in Germaniâ, cum morbo vulgo dicto conſumptione vel febri hectica *laborarent, hoc eo lubentius facio, quod exindè ope principiorum artis medicæ forſitan poteris aliqua deducere quæ humano generi plurimum proderunt.*

Notandum imprimis academiam illam, in quâ ego ipſe per plures annos habitavi, in vertice alti montis prope arcem civitatis Leodienſis eſſe ſitam, et aerem ibi eſſe tam purum ut in eo moniales Anglæ, quæ propè academiam

miam olim habitabant, ut plurimùm pulmonum consumptione vitam amiserint, et ob illam causam in infimâ urbis parte, prope Mosam flumen, in denso aere domicilium fixerint, ubi raro consumptione laborant. Exindè partim et ex aliis observationibus mos invaluit in academiâ, adolescentes consumptione laborantes ad loca infima, nebulosa, et paludosa Flandriæ Austriacæ mittere, Brugas puta, Antwerpiam, vel Gandavum, ubi ope densioris aeris intrà paucos menses sanitatem fermè semper recuperabant; si verò ad academiam redibant, iterùm in eundem morbum incidebant. Si cui verò in mentem venerit dubitare an consumptione propriè dictâ laboraverint, ex sequentibus symptomatibus sive indiciis poterit ferre judicium. Laborabant initio morbi tussi fermè continuâ, deinde sputa sanguinolenta mittebant, tum purulenta et fœtida, macilentiores quotidiè evadebant, pallidus illis erat ut plurimùm vultus, sed interdum subito roseo colore diffusus, oculi ut plurimum vividi et acuti.

Hæc pauca currente calamo, sine terminorum technicorum ornatû conscripsi; sed si crudus hic scribendi modus displiceat, nihil vetat quominus possis ea, non mutato sensû, in meliorem formam redigere. Vive, vale, vir doctissime, et epistolâ hâc, siquid prodesse possit, rutere.

C. LESLIE.

Oxonii, die 7° Sept. anni 1792.

THE

THE following *conjecture concerning the use and effect of manure*, is part of a paper read before the Chemical Society at Edinburgh, April 7, 1786, and printed in that very excellent miscellany, the Edinburgh magazine, for one of the two following months. The theory advanced in it still appears to me, so far as it goes, to be just, and as it is so much akin to the speculations contained in the preceding pages, I have subjoined it here.—

The result of Dr. Ingenhousz's experiments on vegetables exposed to the light of the sun is well known. Since the publication of his English work, he has been more or less constantly employed on the same subject; and on occasion of some controversies, has published both in French and German many experiments, all tending to the same conclusion. His chief controversy was with Mr. Senebier of Geneva, which, however, has terminated completely in his favour, for his antagonist has publicly acknowledged that he was totally misled by some inattention in conducting his processes. I find too in the Acta Theodoro-Palatina (Vol. V. 1784,) a very long series of experiments by Professor Succow of Mannheim, which exactly coincide with those of Dr. Ingenhousz.

He concludes his account of them in the following manner. 'Those effects of the solar light on plants, which Dr. Ingenhousz first so admirably pointed out, are confirmed by the preceding experiments, in which trees and plants appeared most capable of yielding pure air in the light of the sun; whereas in the shade they afforded air more or less phlogisticated. That the air which is extricated when

when vegetables are expoſed in water to the ſunſhine, proceeds from their leaves and other parts, ſcarce needs any proof. Water indeed does contain a quantity of air which is diſengaged by the influence of light; but the quantity is ſo ſparing even in a large quantity of water, that it can by no means be ſet in competition with that which vegetables yield in the courſe of a few hours. Did this air proceed from the water, it would in very few caſes prove ſo pure, unleſs the water contained ſome of the green conferva; but then it would be to this moſs that the origin of the air muſt be aſcribed. The difference in the air itſelf which vegetables yield when other circumſtances are alike, puts it beyond all doubt, that the air muſt proceed, not from the water, but the vegetables.' I ſhall leave the ſociety to judge how many negative reſults will be requiſite to deſtroy the force of theſe numerous experiments, and proceed to what I have more particularly in view.

Dr. Ingenhouſz, in his laſt work, (*Verm. Schr. p.* 394.) has related ſome variations of his experiments. He found that water impregnated with acids, alkalis, neutral ſalts, expreſſed juices of vegetables, as of raiſins, peaches, &c. very much promoted the production of pure air by vegetables, except in the caſe of the graſſes (of which the product is variable from undiſcovered cauſes), of the conferva rivularis, and water-plants in general, which are killed by fixed air, and ſome others, when this acid is employed. The difference is ſo ſtriking that it will be proper to ſelect ſome inſtances.

	Quantity.	Quality.
3½ cubic inches of a triplex lacinia ata in common water . . .	3	228
——— in ſtrongly aërated water	9½	286
but ½ was fixed air from the water.		
3 cubic inches of yew, . .	4	206
——— in aerated water .	19	244
of which the pure air was	12	
2 cubic inches of graſs in 86 of common water . . .	5	280
——— in 86 of aërated water .	13	336
N. B. ⅙ was fixed air.		
2 cubic inches of yew in common water . . .	2¾	225
——— in aërated water .	20	322
⅓ fixed air.		

* In 6 hours of pretty fair weather.

3 cubic inches of yew in 120	Quantity of Pure Air.	Fixed Air.	Quality.
of common water . .	2½	0	234
Ditto with the addition of			
2 cubic inches of muſt .	5	a little	270
with 2 of peach juice	5	a little	292
with 2 of ripe apple .	3½	½	250
with 50 drops of aqua-fortis . . .	4½	⅓	250

** 6 hours of bright ſun-ſhine.

2 cubic inches of graſs in 86 cubic inches of water .	4	0	248

2 cubic

	Quantity of Pure Air.	Fixed Air.	Quality.
2 cubic inches of graſs with 1 cubic inch of muſt .	9½	a little	312
Ditto of ripe cucumber juice	2¾	a little	230
40 drops of vitriolic acid	7½	⅛	295
a little vitriolated tartar	12	¼	296
*** fine ſun-ſhine, 6 hours.			
2 cubic inches of ſempervivum tectorium in 86 of water	3	ſcarce any	245
Ditto with 1 cubic inch of its own juice . . .	4¾	0	308
**** fine weather, 6 hours.			
2 cubic inches of graſs in 86 of water	3	ſcarce any	245
Ditto with ½ cubic inch of ſtrong vinegar . . .	7½	⅛	276
with 1 of juice of the *cornus maſcula* . .	8¼	⅛	323
1 of juice of onion	2¾	⅛	228
1 of juice of turnep	2¾	¼	217

In theſe numerous inſtances, the effect of the addition equally appears from the numbers that expreſs the quantity and thoſe that expreſs the quality of the air produced. It may be obſerved alſo, that a few ſubſtances, ſuch as the juice of onions, cucumbers, and turneps, prevent, inſtead of forwarding, the extrication of air, and that it is of an inferior quality to that which is produced when no addition is made.

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In the firſt place it is evident, that among the ſubſtances which favour the extrication of pure air, we have every thing which can well be ſuppoſed to enter into the compoſition of manure, ſalts ſimple and compound, with the juices and extractive matter of plants. If we may likewiſe aſſume, that the production of this ſalutary fluid is a natural function and an healthy proceſs, it follows directly, that the uſe of manure is to occaſion a greater exertion of that function.

That the production of dephlogiſticated air is among the chief functions of vegetables is a ſuppoſition countenanced by many experiments. Mr. Cavendiſh himſelf infers, that the vital air obtained by Dr. Ingenhouſz comes from the decompoſition of water. There is one experiment related by the laſt-mentioned author, highly remarkable, and not to be explained on any other ſuppoſition that has been hitherto thrown out. 'I boiled, (ſays he l. c. p. 198. B. 2.) ſome water for two hours, and then poured it boiling into a glaſs balloon of the capacity of 200 cubic inches. The balloon was then carefully cloſed. Before the water was grown quite cold, I introduced into the balloon four cubic inches of granulated green matter, which was taken out of the great reſervoir in the botanic garden (at Vienna), and repeatedly waſhed in boiling water; care being taken to ſqueeze out after each waſhing all the moiſture, in order that none except boiling water might remain adhering to it. I next cloſed the balloon with a perforated ſtopple, in order to allow the water an exit when it ſhould be preſſed by the air evolved from the green matter. The balloon was inverted into a veſſel of quickſilver placed in the

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the ſun. The air generated at firſt was abſorbed by the water itſelf: but being ſoon ſaturated, it refuſed to take up any more; and in the courſe of a few days I found a conſiderable quantity of air collected.' If it then be true that water is decompounded by vegetables, it follows that inflammable air is abſorbed and fixed; an opinion countenanced by Prieſtley's experiments on charcoal, on ſliced roots of onions, &c. for neither he nor Senebier, nor Ingenhouſz, nor I think any other, has ever found inflammable air in the elaſtic fluid afforded by the leaves and other proper parts of vegetables expoſed to the ſun. The inflammable matter which ſurrounds certain vegetables is, I ſuppoſe, an eſſential oil in the ſtate of vapour; and if any plant ſhould yield inflammable air in the way I have mentioned, I doubt not but it would furniſh, in other reſpects as well as in this, a ſingular exception to the reſt of the kingdom.

There is ſtill another corollary more preciſe and ſatisfactory to be drawn from theſe premiſes. The quantity of dephlogiſticated air that is extricated, will afford a teſt of the quantity of food taken in by the plant.

It may, moreover, be ſuppoſed, that the additions, which Dr. Ingenhouſz made uſe of, are not thoſe which will produce the greateſt effect. It is reaſonable to ſuppoſe, that Nature in the immenſity of her ſtores, has ſtimulants far exceeding theſe in power; which further inquiry will both diſcover and teach how to apply. For if theſe principles be juſt, they will be eaſily applicable, when we are in poſſeſſion of a greater number of facts, both to gardening

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gardening and agriculture: And I doubt not but that in time a rational ſyſtem of vegetable medicine may be conſtructed, if the ſubject be properly proſecuted. In the mean time, languiſhing trees may be waſhed or ſprinkled with water acidulated with vitriolic acid, which Ingenhouſz found to be moſt effectual in promoting the production of pure air.

It will not be difficult for any perſon who may chooſe to reflect on the ſubject, to contrive other experiments, by which theſe principles may be confirmed or refuted.

FINIS.

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ERRATA:

P. 3. l. 7, dele *all.*—p. 27. l. 10, for *alkaliæ*, read *alkali.*—l. 54, for *him*, r. Dr. *Trotter.*—p. 96, for *graſs*, r. *gras.*—p. 108. l. 5, between *azote* and *from*, inſert *abſorbed.*—p. 118. l. 22, for *is*, r. *ſeems.*—p. 123. l. 26, inſtead of "*no craſſamentum from*," r. "*little diſpoſition to coagulate in.*"—l. 27, for "*none from*," r. "*the ſame.*"—p. 61. l. 6, dele *he.*

I know how much MILLMAN inſiſts upon the craſſamentum, obſerved by ſome authors in ſcorbutic blood. But in ſuch a caſe I think a very few *negative* more than counterbalance thouſands of *poſitive* teſtimonies, where no account is given of circumſtances, and no attention has been probably paid to them: the very mode of blood-letting, the time it ſtands before it is examined, the temperature in which it is kept, &c. may affect the combination of blood with oxygene, and by conſequence, its coagulation.

It is perhaps, in the preſent improved ſtate of chemiſtry, practicable to aſcertain the laws of the coagulation of the blood: Mr. PARMENTIER and DEYEUX, have not indeed perfectly ſucceeded in their reſearches on milk, a very ſimilar fluid. It is remarkable, that in their experiments the heat of boiling water would not curdle or produce a ſkin upon milk without the preſence of air, and yet that it ſhould have been indifferent what kind of air was preſent.

P. 144. l. 22, for *oxygene*, r. *hydrogene.*—p. 151. l. 22, for *are*, r. *is.*—p. 152. l. 20, for *pains*, r. *pain.*—There will perhaps occur a few inaccuracies beſides theſe, but no reader can be at a loſs how to correct them.

N. B. I have lately attended to the colour of phthiſical blood; and in ſome inſtances where it was juſt drawn, I have perceived both the *florid* and *claret* or *purple* colour; the former is diſtinctly ſeen, when the blood is ſpread thin, the latter, when it has a conſiderable depth. This will explain the apparent contradiction in p. 124.

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